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# CURRENT TRENDS IN PSYCHOLOGY
# AND THE BEHAVIORAL SCIENCES

Six lectures
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# CURRENT TRENDS IN PSYCHOLOGY AND THE BEHAVIORAL SCIENCES

JOHN T. WILSON
CLELLAN S. FORD
B. F. SKINNER
GUSTAV BERGMANN
FRANK A. BEACH
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1954
UNIVERSITY OF PITTSBURGH PRESS

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# PSYCHOLOGY AND THE BEHAVIORAL SCIENCES

INTRODUCTION

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# INTRODUCTION

### ROBERT A. PATTON

It is my pleasure to introduce the participants in the eighth Annual Conference on Current Trends in Psychology. The topic which was chosen this year concerns the relationships between psychology and some of the other behavioral sciences.

Specialized fields of scientific endeavor are continually growing and invading each other's territory. Work is being duplicated and some of the most important ideas receive different names from each group. These boundary regions of science, however, offer great rewards to the trained investigator. As Wiener remarked in the introduction to "Cybernetics," if a physiologist who knows no mathematics works together with a mathematician who knows no physiology, the one will not be able to state his problem in terms the other can utilize, and the second will be unable to put the answers in any form the first can understand.

Some of the problems encountered in areas where fields of science overlap can be illustrated by taking concepts currently receiving attention and showing the contributions which have been made from different directions.

The word "stress" has been widely used and appears to have been all things to all investigators. Research workers from many different areas have joined in the study of stress effects upon behavior. Examining and defining the concept will illustrate some of the problems which make this conference desirable. Stress studies have brought together many different workers and yet no entirely satisfactory definition of psychological stress is available.

Behaving organisms are elaborate energy systems which are capable of responding to environmental changes, many of which may threaten the constancy of internal steady states. They are composed basically of organic building blocks which are in themselves of very unstable composition. The relationships among these substances are characterized by an ebb and flow of energy transformation in which nutrient materials are broken down in orderly sequence by means of various enzyme systems. Although constantly in flux the internal environment of living creatures preserves an amazing constancy in the face of changing and often adverse conditions in the environment. In effect the relatively automatic control over internal conditions has freed the organism for more complex behavioral adjustments to its environment. The significance of such internal steady states as visualized by Bernard in 1878 has been more recently investigated by Cannon and his collaborators.

In discussing the problems involved in the study of stress effects the individual will be considered as an integrated and self-regulating group of systems of various levels of behavioral complexity ranging from physiological needs to acquired psychological drives such as the necessity for self-esteem and the need for acceptance. At each level the individual tends to maintain a relatively stable condition of equilibrium or adjustment. In the face of stressful experiences such as military combat these behavioral patterns may be threatened not only by the presence of actual danger, but more importantly by man's capacity to react to threats or symbols of danger derived from his past experience. Reactions to *symbols* of danger may be similar to those involved in actual assault. In this way protective behavioral mechanisms must be developed to anticipate stress as well as to meet it. Man's reactions to environmental threats involve both physiological reactions as well as far more subtle anticipatory behavior which we name "tension" or "anxiety." Man's highest functions in foresight and planning have rendered him particularly vulnerable to anxious anticipation of real or imagined threats to his well-being. The interdependence of various levels of dynamic adjustment to environmental threats has made the investigation of stress effects on behavior a challenging and difficult area for research.

## THE CONCEPT OF STRESS

Modern concepts of behavioral stress bear the imprint of earlier ideas developed in the field of engineering as well as in physiology. As used by the physicist or engineer, stress has been defined in terms of externally applied forces; mutual force or action between forces or bodies; or resistance to alteration in form. This sort of definition, however, raises great difficulties when we try to apply it to the behavior of living organisms. Some of these difficulties may be considered briefly.

### *Stress as a Condition of Stimulation*

Stress may be thought of in terms of certain specific environmental conditions to which the organism is exposed. For example, a certain degree of oxygen lack may be imposed on a group of subjects in a pressure tank. When behavioral measurements are made, however, it has been found that similar variations in the external stimulating conditions result in extremely diverse reactions on the part of the subjects. This has been even more apparent when examinations or threats of failure have been used as stressful conditions. It seems impossible to predict the deviations in behavioral responses from a definition of external stimulus conditions. This is the source of difficulties which makes it necessary to part company with the engineer's conception of stress with which we began. Since organisms are a product of all stresses to which they have ever been exposed, they may be expected to differ greatly in their individual reactions at a given time. The understanding of perceived meanings or individual motivation receives little help from mechanistic conceptions of stress.

### *Stress as a Response of the Organism*

Another possibility which suggests itself is to define stress in terms of what the subject does rather than in terms of what is done to him. This, too, leads to great difficulties. What should be measured? So-called psychological stress studies have been for the most part a vain "search for dependent variables." Should one measure the subject's performance on a skilled task, or what he says he feels? These measures may differ widely. Many individuals may tremble, sweat, and show respiratory irregularity while achieving a good score on various tests or examinations.

It is difficult to devise a satisfactory concept of stress from either stimuli or responses, and the idea therefore falls between, as an intervening variable. Most investigators seem to have considered tacitly as stressful those conditions or situations which have threatened or prevented the attainment of some goal. It is then immediately necessary to introduce motivational concepts, since behavioral responses are always a function of an organism's past and of the way the individual perceives his goals. All of which is to say that the stimulus-response paradigm does not help us much in dealing with the behavior of individuals responding to complex environmental stimuli.

### *Physiological Conceptions of Stress*

Our ideas about stress have also derived from the field of physiology. Cannon's conception of homeostatic steady states referred to the facility with which an organism preserves its physicochemical equilibria in the face of changing environmental conditions (stresses). Homeostatic mechanisms as studied by Cannon are relatively short-term adjustments and are made in a more or less automatic fashion. The comparative ability of a given organism in carrying out these minute physiological adjustments is one measure of its "freedom" from its physical environment.

In physiology considerable progress has been made in the development of operational definitions of stress. There are certain reasons for this. The physiologist can often define stimulating conditions (pH, drug, potency, temperature, 0 level) quite accurately. It is also possible for him to measure certain responses accurately, i.e., blood chemistry, respiratory quotient, or endocrine effects. Selye and his co-workers have found that widely diverse environmental stresses may produce a very uniform pattern of physiological responses. He has named this pattern the "general adaptation syndrome" and it has become the most recent and comprehensive formulation of physiological stress concepts. These ideas have had only limited application to problems involving psychological effects of stress upon behavior.

It has been necessary, therefore, to arrive at some definition, and the following one has been chosen: psychological stress is the perception of threat and the attendant disruption of homeostatic behavior patterns. The definition implies the direction which future research may take. One must study the variations which individuals show in their perceptions of threat-producing situations. Secondly, it will be necessary to investigate the normal variations (homeostasis) of behavioral processes chosen for study. Stress can then be defined in terms of the stimulating conditions which cause a given individual's behavioral system to: (a) oscillate beyond its normal bounds of variation, and (b) return more slowly to its mean. My point in the preceding remarks has been to illustrate the problems involved in defining terms used by scientists working in boundary areas. The speakers who follow should clarify some specific problems and describe important work in areas linking psychology to other scientific areas.

# PSYCHOLOGY AND BEHAVIORAL SCIENCE

### JOHN T. WILSON

IN the closing pages of his *Principles of Behavior,* the late Professor Clark L. Hull summarized his efforts and projected a glance into the future of what he termed "behavior science" by saying:

> In so far as these principles . . . are sound and sufficient, it should be possible to deduce from them an extensive logical hierarchy of secondary principles which will exactly parallel all of the objectively observable phenomena of the behavior of higher organisms . . .
>
> It is hoped that as the years go by, systematic treatises on the different aspects of the behavior sciences will appear. One of the first of these would naturally present a general theory of individual behavior; another a general theory of social behavior. In the elaboration of various subdivisions and combinations of these volumes there would develop a systematic series of works dealing with . . . the theory of skills . . . ; of communicational symbolism or language; of the use of symbolism in individual problem solution . . . ; of social or ritualistic symbolism; of economic values and valuation; of moral values and valuation; of aesthetic values and valuation; of familial behavior; of individual adaptive efficiency; of the formal educative process; of psychogenic disorders; of social control and delinquency; of character and personality; of culture and acculturation; of magic and religious practices; of custom, law, and jurisprudence; of politics and government; and of many other specialized behavior fields.[19, pp.398-399]

After expressing this article of faith in the eventual extension of the scientific continuum to include a rational explanation of even the most complex forms of human behavior, Professor Hull returns his readers to reality by stating that in his judgment, it would be a long time before the "behavior sciences" attained the status that he was contemplating; that the task would be both arduous and exacting; and that it would necessitate a departure from the traditional "belles-lettres" and brilliant conversational approach so prevalent in the social sciences to one of rigorous quantitative experiment.

In the ten-year period since the appearance of *The Principles,* there has been an unprecedented ferment along that segment of the scientific spectrum encompassed by Professor Hull's conception of "behavior science." The ferment has involved both research and, to a lesser extent, theoretical development, some of which has been accomplished within the framework of traditional scientific disciplines, and some on the basis of interdisciplinary collaboration. One could find varying opinions regarding the extent to which these activities have contributed to a science of human behavior and to the attainment of the goal described by Professor Hull, but that they have been instrumental in lowering many of the barriers between traditional academic disciplines, and in encouraging interaction between psychology and certain social science fields would not be seriously questioned. What were at one time neglected or ignored research areas between the study of individual behavior, on the one hand, and the study of social and cultural behavior, on the other, have become the common meeting ground for psychologists and their behaviorally oriented social science colleagues to an extent which may well foreshadow the emergence of a unified science of human behavior.

## BACKGROUND OF THE RELATIONSHIP BETWEEN PSYCHOLOGY AND BEHAVIORAL SCIENCE

The contribution which scientific psychology has made to this emerging behavioral science is, of course, a direct function of the extent to which it has, throughout its history, devised adequate concepts and techniques to handle successfully not only problems of individual behavior, but problems of collective behavior. The rapid and somewhat turbulent expansion in the content of psychology within the past few years might lead one to believe that its broadening scope is a development of recent vintage, but one need only to recall Wundt's ten-volume *Völkerpsychologie* to realize that an underlying motivation for such a development has been present in psychology from its beginning.

As is the case with almost any facet of American psychology, a trace of it can be found in William James, and the press of psychology toward a more general behavioral science is no exception. Had you participated in a meeting of the Harvard Psychology Club in the fall of 1906, you could have heard James state that as a student of psychology he had:

> . . . always regarded it as but a part of the larger science of living things. Official psychology is a very *small* part. There should be a much larger functional psychology which should discover the forces governing the moral and religious life of man and develop a technique for controlling

them. Laboratory psychology may be more accurate at present, but its program makes it look small.[37, II, p.121]

And both McDougall and Hall very early echoed this belief in urging that psychology should make itself serviceable to the social sciences by inquiring into the motives of social behavior.[15,33]

The degree to which psychology has moved over the years in this direction is reflected in Fernberger's review of the American Psychological Association from 1892 to 1942.[10] To describe the general trend of research interests of psychologists as indicated by papers given at association meetings, Fernberger categorized research reports into two groups, one of which he labeled "academic" and the other "humanitarian." The first of these two groups included papers on general psychological topics, on perception, on learning, memory and thinking, on neurological-physiological reactions, and on animal psychology. It was characterized as having as its central theme the advancement of psychology as science. The second group included papers on social psychology, clinical and abnormal psychology, and on various phases of the application of psychology to human affairs. These presumably had at heart the welfare of society.

Interestingly enough, at no time from the beginning of the APA has there been a meeting at which papers of both kinds were not presented. Until 1909 the number of "academic" papers greatly exceeded the number of "humanitarian" papers, but during this time the latter slowly increased. From 1910 to 1939 the balance remained about even, with periods of superiority in numbers of papers given alternating between the two groups. From 1940 on, however, there has been an increasing disparity between the frequency of papers in the two categories, with a sharp advantage going to the "humanitarian" group.

In discussing this trend Fernberger mentions a point which is of interest. As an illustration of how times of stress in our society influence psychology toward problems of social import, he makes reference to a paper given by Jastrow at the twenty-fifth anniversary meeting of the APA, which was held during the Christmas holidays of 1916. In this paper Jastrow reminded his fellow psychologists, that:

> . . . we of today are witnessing the largest and most appalling issues of estrangement in ideals, sentiments, allegiances, that the world has faced; the psychology of war must be considered in the establishment of enduring human relations. The world is going to be wisely ruled, the endeavors of organized men more sanely directed, the errors of the past less disastrously repeated, if a body of men find participation in the direction of affairs possessed of a psychological discernment; for this insight is as indispensable to modern conditions in certain relations as is an economical, a political or a business sense in others.[21, p.263]

We have no way of estimating the extent to which such urgings on the part of individual psychologists altered the course of psychology away from purely objective studies of individual behavior and toward a consideration of man in his social environment, but the effect of World War I in bringing psychologists out of the laboratory and into the world around them is well known. The lasting impact of this experience upon the growing social consciousness of scientific psychology would be difficult to assess, but that there was a residual influence might well be inferred from the fact that by the time of World War II, some twenty-five years later, psychologists were eminently successful in meeting a wide variety of responsibilities, ranging from those at the level of pure laboratory psychology to those involving problems of interpersonal and sometimes international relations.

What happened in psychology during the "between-wars period" to further its understanding of and interest in problems of human social behavior? In their review of the evolution of our current conception of man and of methods for studying him, Hilgard and Lerner[23] mention several influencing factors. They point out that the writings of Freud, despite his own disavowal of himself as a social therapist,[12] were of primary importance in that they elevated motivational concepts to their current position of central importance in psychology. The manner in which such concepts have influenced psychology, from clinical to experimental, is reflected in Sears's survey of objective studies of psychoanalytic concepts[42] and again in his more recent review of the field of personality.[44] The merging of personality concepts and theory with general psychology, has also been a factor in broadening its scope.[1] The extent to which such personalistic psychological concepts have become a part of general psychology is evident in the survey of research in perception by Zener *et al* [51] as well as in the report of a recent interdisciplinary congress on culture and personality.[3] The work of Lewin most certainly should be mentioned as an influence in extending the frontiers of psychology to include the experimental study of social behavior.[24,25,28] And, the activity of the Hawthorne group, under the direction of Mayo and his colleagues,[31] while not as much in the central stream of psychology as the other influences that we have mentioned, was a landmark which heralded the coming interaction between the psychological and the human relations approaches to the study of collective behavior. These and other examples which could have been chosen (the work of Allport, Murray, Moreno, Sherif, and Cantril, to name but a few) illustrate the rumblings within psychology during this period.

Certain developments of a nonsubstantive nature, made possible largely through the support of private foundations, were also taking place to provide agencies and mechanisms within which interdisciplinary actions could occur. One of these was the establishment in 1924 of the Social Science Research Council which, as some of you may know, is a delegate-type organization of scholars representing seven associated professional societies in the social sciences and closely related disciplines, including political science, economics, sociology, psychology, anthropology, history, and statistics. Through its various committees and programs, especially the Committee on Scientific Method, there has been an enhancement of methodological competence and intercommunication among the social sciences and between psychology and these fields. The rise of the "institute" concept and of departments of social science within the universities also played a role in furthering relationships between psychology and other behavioral sciences. Although taking various organizational forms, such as the Institute of Human Relations at Yale, or the several child experiment stations, they generally have attempted through the technique of special organization, to further the evolution of at least an interrelated if not a unified science of human behavior.

These illustrations, despite their somewhat unsystematic selection, are sufficient to reflect the scientific climate and the course of developing collaboration between psychology and other behavioral science disciplines during the pre-World War II period. World War II brought psychologists and their scientific neighbors closer together than ever before, and through the "interdisciplinary project" and "team research" intensified their interaction. We shall not attempt to review the complex of events during the war years, but will merely indicate that they resulted in an interdisciplinary catalysis which ushered in a period described by Gardner Murphy[34,p.318] ". . . supreme opportunity (for psychology) to relate itself organically to the needs of civilization for which it has arisen, and perhaps to stabilize, strengthen, humanize that civilization."

## SOME CURRENT CONCEPTUAL AND RESEARCH RELATIONSHIPS BETWEEN PSYCHOLOGY AND BEHAVIORAL SCIENCE

Psychologists and their behavioral science colleagues have grasped this opportunity in a variety of ways, and from their efforts have come new conceptions and new lines of research which, along with their more traditional counterparts, define the relationships between psychology and behavioral science as we find them today.

On at least two recent occasions, stemming from opportunities which afforded relatively large-scale financial support of research, psychologists and their scientific colleagues have jointly drawn up programs aimed at defining, in terms of common problem areas, the dimensions of an interdisciplinary approach to the study of human behavior. Such statements should, of course, be viewed as planning documents, rather than as final and irrevocable pronouncements, but insofar as they reflect the interrelationships between psychology and other behavioral sciences, they are germane to our discussion.

The first of these two programs was drawn up late in 1946, when the Office of Naval Research (then the Office of Research and Inventions) brought together a group of civilian consultants for the purpose of developing a program of research in the broad category of the behavioral sciences, which was to be an integral part of ONR's research-support effort. This advisory panel consisted initially of representatives from anthropology, economics, political science, sociology, psychiatry, and psychology, and although panel membership shifted throughout its history, it continued to maintain a high order of interdisciplinary representation. The early history and experience of this group have been described graphically by Darley,[14] and it would appear that it might well have been the subject of an interesting study in group behavior. But despite difficulties in communication and in conciliation of divergent viewpoints, the panel did evolve the framework of a research program encompassing the following problem areas:

1. *Comparative study of different cultures:* To provide a basis for understanding the behavior and goals of groups, (including) research in the economic, political, cultural, psychological, and sociological structure of nationality groups . . .

2. *Structure and function of groups:* . . . the research to study the productivity, structure, and development of . . . groups in relation to . . . assigned tasks so that we can arrive at more effective selection, training, and management procedures for group relations . . .

3. *Problems of communication of ideas, policies, and values:* (Because) between nations, between groups within a nation, and between individuals within a group, the effectiveness of communication is of paramount importance.

4. *Leadership:* Just as all individuals at some time must operate within a group, so these groups operate under various forms of leadership. (The leader's) . . . contribution is often a determining factor in the group's effectiveness . . .

5. *Growth and development of the individual:* no overall pattern of research in human relations can provide workable answers unless it includes studies that are focussed on the development of individuals' capacity to participate in group life . . . [14, pp.6-7]

To a large extent the later programs of the other military agencies, and the committee and panel structure of the Research and Development Board, in this fifth area of "Human Resources" research followed the pattern developed by ONR and reflect the same relationships between psychology and other behavioral science disciplines. The research-support program of the National Institute of Mental Health, which was established in 1946,[35] also reflects in some respects similar relationships between psychology, psychiatry, and sociology.

The second large-scale effort to interrelate psychology and other behavioral sciences is that of the Behavioral Sciences Division of the Ford Foundation. In 1951 the Foundation announced that it would support:

> . . . scientific activities designed to increase knowledge of factors which influence or determine human conduct, and to extend such knowledge for the maximum benefit of individuals and of society.[39, p.7]

In elaboration of this statement, the trustees of the foundation authorized a program which had been drawn up by officers of the foundation, in collaboration with consultants from psychology and various social science disciplines, and which covered these study areas:

> *Political behavior:* (including) . . . such questions as the causes and consequences of political participation and apathy in democratic society, the distribution of political values and doctrines, and the characteristics and codes of political leaders.
>
> *Communication:* The term . . . deals with such questions as the best means for international communication, the effects of communication media upon different kinds of people, and the appropriate channels for distributing ideas.
>
> *Values and Beliefs:* . . . Such research questions are involved as the relationship between consciously professed values and implicit values, the process by which values change, and the factors producing conformity and nonconformity in a society.
>
> *Individual Growth, Development and Adjustment:* This

topic is concerned with the whole complex of biological, physiological, cultural, and psychological factors that account for personality formation and change . . .

*Behavior in Primary Groups and Formal Organizations:* (Such) . . . research questions involve the extent to which . . . groups influence the decisions of the individual, the way in which the individual resolves conflicting group interests, and efficient modes of administration within large-scale organizations.

*Behavioral Aspects of the Economic System:* . . . Typical problems deal with the psychological factors influencing the productivity of workers, the opportunities provided by the economic system for occupational and social mobility, and the psychological and social effects of mass production tasks in large-scale industry.

*Social Classes and Minority Groups:* . . . Illustrative questions here have to do with the movement of individuals within the class structure, the effect of such characteristics upon political activity, and the ways of maintaining healthy relations among ethnic groups.

*Social Restraints on Behavior:* . . . (including) such questions . . . as the effect of technological development upon social control, the effect of legal rules upon actual behavior, and the relation between social control and individual freedom in a period of international tension.

*Social and Cultural Change:* This topic refers to . . . the social and psychological effects of economic development in so-called under-developed areas, the means by which people adjust themselves to rapid social change, and the effect of military developments on social life.[39, pp.18-20]

It is evident from these sets of statements that, at least in the minds of the individuals composing them, the relationships between psychology and other behavioral sciences, in terms of common problem areas, encompass an extremely wide scope.

As a part of the current psychological research scene, one can find activities which are directed toward problems spanning no less a breadth in scope than the foregoing programmatic statements. Although to be found throughout the whole field of psychology, the foci of these efforts are generally in the areas of social psychology and the psychology of personality. They range in substance from research which is aimed at extending traditional scientific psychological concepts into a context of social behavior (or conversely translating terms from social science fields to more behaviorally flavored concepts) to what might be called "psychologizing" traditional social science disciplines.

A series of experimental studies by Bruner, Postman, and their colleagues,[6,38] for example, illustrates the extension of the concept of perception to include the manner in which needs, internalized values, and various forms of personal stress affect the emphasis of different segments of the perceptual field. In a recent theoretical formulation, Bruner[4] has further extrapolated the concept to cover perception in more general social situations. And, the work of McClelland and his collaborators,[32] in their studies of the influence of needs on fantasy production, represents an experimental investigation of motivational concepts within a theoretical framework which eventually may prove to be effective in bettering our understanding of motives as they relate to socially adaptive behavior. In a somewhat different way, the studies of Blum[5] which have been aimed at clarifying the syntactical relationships within psychoanalytic theory, illustrate a potential contribution of psychology to general behavioral science, in that clarification of psychoanalytic concepts, if successful, would permit a somewhat more comfortable application of such theory in cross-cultural investigations than has been possible heretofore.

Current research on group processes probably reflects better than any other single area the merging of psychology with other behavioral sciences. In a limited sense these relationships were defined in Lewin's two early papers on "group dynamics,"[26,27] but they are reflected more generally in the content of recent research on social behavior. Included within such research one finds studies on: the theoretical conceptualization of group structure and function; the effect of various group influences upon both individual and group productivity and action; the social psychology of public housing; behavioral contagion in groups of disturbed children in therapy camps; the influence of "democratic" as contrasted to "autocratic" organization and management within groups; the consequences of deindividuation within groups; the effect of conformity pressures on individuals of differing ethnic background; studies of tensions affecting international understandings; personal factors in both leaders and in those being led; the effect of group stratification upon the social and political behavior of members of the group; the effect of certain characteristics of the social group upon the incidence of juvenile delinquency; and so on.[9,44,45,46,47,48]

At a somewhat different level than is illustrated by the research that we have been describing, the growing interrelationships between psychology and other behavioral science fields are reflected in the extensions, by various individuals, of psychological concepts into such social science fields as economics, political science, and sociology. For example, Hayes,[16] in an extensive review, has pointed out the relationships of psychology to economics and has suggested contributions which might be anticipated from a psychological approach to certain types of economic research and theorizing. Also, the work of Katona[22] in this same area, is an interesting illustration of the adaptation of psychological methods to the study of factors influencing consumer behavior, business decisions, and economic fluctuations. Of interest to political scientists is Bauer's[2] account of the changing conception of human nature on the part of Soviet nationals in which he attempts a psychological analysis of ideological and political issues in a selected national group. At a purely theoretical level, Tolman[50] has essayed a clarification of the relationships between sociological theory, psychological theory, social psychology, and psychological sociology in his analysis of these areas in terms of the comparative nature of their independent, intervening, and dependent variables. And, on an even broader scope, Tolman and others have attempted to develop a general theory of social action.[36]

Finally, in our consideration of current research and conceptual relationships between psychology and behavioral science, the influence of an early and continuing concern with methodological problems should not be overlooked. The importance of devising research methods which are adaptable to the peculiar needs of the social sciences is indicated by the amount of emphasis put on such activity by the Social Science Research Council, through its Committee on Scientific Method. The work of this committee resulted in 1931 in a volume, edited by Stuart Rice and entitled *Methods in Social Science: A Casebook.*[41] Chapters by Allport and by Young reflect the early interaction between psychological research methods and those in social science fields. The technique of the sample survey, which played such an important role in wartime attitude research and in various programs since; the utilization of psychological techniques in anthropological investigations; the development of mathematical models and other quantitative techniques for handling behavioral data; all attest to the persisting influence of methodology as a dimension along which relationships between psychology and other behavioral sciences will continue to develop.[11,20,29]

## SUMMARY THOUGHTS IN REGARD TO PSYCHOLOGY AND BEHAVIORAL SCIENCE

In summarizing our discussion of the relationships between psychology and behavioral science, one thing we can say with certainty is that a great deal is going on in the way of interdisciplinary activity. In both its research and its theoretical phases, the activity might be characterized as a search for common dimensions which will serve to conceptualize behavioral problems at varying levels of complexity from individual to social, as well as to reduce them to manageable experimental enterprises. It is sometimes difficult to discover much order in any given area of activity; and, it is frequently impossible to determine just what the relationships between different lines of development are. With reference to methods, new and improved research techniques are making their appearance, and with better methods one can hope for more adequate and more systematic handling of the important variables which characterize collective behavior. In general, one may say that in terms of Professor Hull's projection, there is a growing body of evidence that the study of social behavior is departing from a conversational approach to one of quantitative experiment.

Such interdisciplinary developments are not without their problems, however, and to leaven the foregoing note of optimism just a bit, we should like to mention two or three which appear to us to be rather fundamental, if interactions which are optimal to the development of a healthy behavior science are to be initiated and maintained. First of all, and perhaps most fundamental, there seems to us to be a need to consider carefully the implications of certain conceptions of "behavioral science." Terms such as "behavior science" and the "behavioral sciences" have been appearing with increasing frequency in psychological and social science literature. Different authors have used them in various ways but not always consistently. Some use the terms in a generic sense, much as the term "biological sciences" is often used to signify a collective grouping of biological science disciplines.[8] Others have employed them to signify a limited grouping of certain specific social science disciplines, usually social psychology, sociology, and cultural anthropology.[18] In Ford Foundation usage, it has been carefully pointed out that the term is not the equivalent of any group of social sciences; that it does not include all of psychology (for example, it omits physiological psychology); but that it is meant "... to denote all those intellectual activities that contribute more or less directly to the scientific understanding of problems of individual behavior and human relations."[39, p.13] At the University of Chicago, "behavioral sciences" refers to "... all the overlapping biological and social science disciplines concerned with the study of behavior, human or subhuman."[49, p.1]

Insofar as categories are useful, we have found the concept "life science" more helpful in getting across to physical scientists and to nonscientists the common dimension relating problems of interest to biologists, psychologists, medical, and social scientists.

Now we do not wish to labor the matter of definition, although a degree of consistency might reasonably be expected from scientists when such terms are used or referred to. A more subtle problem insofar as relationships with psychology and a furtherance of a science of human behavior are concerned, is that within many current conceptions of "behavioral science" there is a strong emphasis on the study of behavior at only the human level, and this largely in a context of very complex social and cultural situations. There is a concomitant lack of emphasis, amounting almost to a rejection of interest, in the study of behavior at simpler levels, either human or subhuman. It seems to us that such an emphasis, in the face of extremely meager knowledge concerning the variables involved in social behavior, and accompanied by means sufficient to seriously influence the direction of training and research in the disciplines involved, may prove to be illusory as the most direct route to our eventual understanding of the phenomena of collective behavior. The point has been well made by Skinner, in a discussion of the study of human behavior as a function of the complex variables operating within the various agencies of social control in our society. He points out that:

> Our conception of human behavior need not be *deduced* from the complex facts which it must eventually explain. We are not required, for example, to discover the characteristics of the political animal through a study of the facts of government. If the political animal is man himself, we may study him elsewhere and often under better conditions. A functional analysis of behavior provides us with a basic conception with which we may approach each of these fields (government; education; economics; etc.) . . . It will then be possible to consider the effect upon the individual of the *total culture,* in which all our controlling agencies and all other features of the social environment work together simultaneously and with a single effect.[43, p.334]

A second, less serious and I believe transient, problem which has marked the interaction between psychology and its behavioral science neighbors is the relaxation in some respects of the standards of scientific rigor normally found in the more firmly established areas within psychology. In connection with this problem it is interesting to note the following statement which appeared in a report of the committee studying the place of psychology in Harvard University:

> . . . the inevitable tendency (of psychology) will be to extend and develop affiliation with the social sciences—sociology, anthropology, and government—though such affiliation does not imply an uncritical acceptance of their

> doctrines . . . the essential points that need to be made are: (1) sociology and psychology are different fields although related in the study of group behavior. (2) The methods used by social psychologists are (a) widely used by sociologists and (b) require very little knowledge of science other than a part of psychology. (3) There is widespread practice of requiring little or no scientific training for budding young social psychologists with the result that the name psychology covers both scientists and non-scientists who do not speak each other's language and who have less of a common scientific background than do geologists and zoologists . . .[40, pp.3-5]

It seems to us that a not unreasonable position is to view the difficulty of maintaining appropriate rigor in behavioral science inquiry as a passing one, and largely a function of the stage of its development. Darley's comment in the summarizing chapter of *Groups, Leadership, and Men* is particularly pertinent. He notes that research on the phenomena of social behavior is inevitable, and that this being the case, we may then demand that it:

> . . . be as carefully formulated and explicitly done as studies of individuals. This does not mean that we favor laboratory research over field research, or "pure" research versus "applied" research. We would only demand that any experiment be carefully done, without immediate concern for pressures to produce, to solve practical problems, (or) to eventuate social change.[14, p.265]

The third and last problem that I should like to mention concerns the mixture of *social engineering and social philosophy* which is frequently reflected in what is avowedly *social or behavioral science.* The problem revolves around the general issue of the control of human social behavior and the relationship of this issue to psychological and behavioral science. Our concern is not with the fact that controlling agencies exist and that they draw upon science for new and improved techniques. Society is full of controlling influences and interests, and obviously, those agencies in positions to exert social control (schools, the courts, and government, for example) should be in possession of the best in social science and, parenthetically, subject to some system of checks and balances themselves, if society is to improve itself. We are concerned, however, with what we believe to be a confusion between this sort of "relying upon" social science by those responsible for effecting social change or control, and "participating in" social science to influence such change or control. It is in respect to "participating in" social science to influence social change that the problems lie, and wherein there is evidence of a mixed self-image on the part of some psychologists and their social science colleagues. To us, this problem comes into clearest focus within the field of psychology in the concept of "action research."[7]

In action research the orientation is toward the study of group dynamics in "real-life" situations, and generally the scientist involved collaborates with a practitioner with a view to attaining a goal which is conceived (presumably by both) as being socially desirable, as, for example, the reduction of intergroup tensions regarding race prejudice, or wages, or perhaps merely interpersonal relations. One of the basic tenets of action research, as stated by Chein, Cook, and Harding, is that the results of the scientist's labors " . . . should be useful and significant, (and) that they 'should make a difference' . . . "[7, p.43] A somewhat similar idea seems to underlie a statement made by Rothwell in the foreword to a new treatise on "the policy sciences" when, after indicating that social scientists have been launching new and revealing inquiries into the complexities of human relations, he says, "We are clarifying our thought about what society *should* be, by postulating explicitly the values upon which it *should* rest and by discovering with more accuracy what that society is today and how it became what it is"[23, p.viii] (*italics* ours).

The obvious ethical problems facing the action researcher have been discussed by Katz[45] and we shall not go into them here. He has also pointed out certain shortcomings of such research settings for deriving general principles which underlie social behavior. In all of the discussion, however, the basic issue of objectivity as a fundamental characteristic of science, and the difference between scientist as scientist, and scientist as social-engineer-in-science seem to have been neglected. The essential point is succinctly made by President Compton of M.I.T. when, in testimony relating to the inclusion of social science within the then pending National Science Foundation Act, he stated that:

> I am somewhat suspicious of any group trying to set out a program of discovery of the facts of nature, as far as fundamental science is concerned, on the basis of an anticipated exploitation or intensification of one or another social objective.[17, p.632; 30, p.404]

Such problems notwithstanding, it is abundantly evident that within the varied and numerous interdisciplinary activities which we have indicated, there has been a sincere and effective *rapprochement* both in substance and in research techniques between psychology and related behavioral science disciplines. It is also evident that the *rapprochement* is intended in the future to go even further.[13] If such developments are successful, and there is reason to believe that they may be, they will represent a striking synthesis of knowledge in the behavioral science domain.

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# SOME POTENTIAL CONTRIBUTIONS OF ANTHROPOLOGY TO PSYCHOLOGY

CLELLAN S. FORD

THE scientific study of human behavior is shared by several disciplines. For many decades they have contributed to one another, more often than not with enlightening results. Within recent years there has been an increasing tempo of interchanges between two of the basic disciplines concerned with human behavior, namely, psychology and anthropology. This cross-fertilization already has produced much of significance for a science of man and promises to contribute a great deal more. It will be the task of this paper to indicate briefly some of the ways in which anthropology can contribute to psychology and to our understanding of human behavior.

From the beginning, anthropology has been a natural history discipline engaged primarily in the description of people, their physical characteristics, their customs and sociopolitical organization, their ideas and their beliefs. Some anthropologists have focused their attention upon the history of mankind and have attempted to reconstruct the vast complexities of human existence, supplementing historical records with the unearthing of archaeological remains. Among those interested in human history have been those who sought to formulate evolutionary principles and developmental processes. Other anthropologists have concerned themselves more specifically with the phenomena of culture change and culture contact, looking closely at such phenomena as invention, diffusion, and acculturation. Still others have been interested in some particular facet of social life and have attempted to analyze from one or another point of view such segments of human living as language, social structure, kinship systems, technology, or political organization. A few have been interested in the relationships between various aspects of human experience, such as between the behavior of the individual and the type of society in which he lives, or between the experiences of early childhood and later adult behavior.

A necessary counterpart of all these interests has been the detailed description and analysis of the customary behavior and the social and political organization of specific cultural and ethnic groups. No matter what particular problem interests the anthropologist especially, one of his primary concerns is to contribute a descriptive ethnography of the people whom he has attempted to study and to understand. The information available in the ethnographic studies already produced by anthropologists has potentially very great significance for psychology. Even greater contributions will be made to psychology by the ethnographers of the future. The reasons for this can be made clear if we examine how ethnographic data are collected.

The descriptions of specific peoples and their culture resulting from ethnographic field research consist predominately of empirical generalizations arrived at by careful observation, intensive interviewing, and elaborate checking. Usually the anthropologist lives for a year or more with the people he studies and has ample opportunity to re-examine over and over again his conclusions concerning the patterns of their behavior. Often he learns the language of the people studied, though he may supplement this means of communication through a lingua franca or by calling upon the services of interpreters. The generalizations with which he returns, and which eventually appear in monograph form, are tested statements describing the behavioral and structural characteristics of the society studied. The anthropologist may have investigated some particular problem, or may have concentrated on some particular aspect of culture, but whatever his special interest he will include in his monograph generalized statements about the culture and structure of the society visited.

The generalizations concerning a people which the anthropologist brings back from the field do not of course describe every facet of behavior. They are not comparable, for instance, to a complete sound-motion-picture recording of the lives or the daily activities of a people. Instead, these generalizations more closely approximate the rules by which the people's game of life is played. Some of these rules are explicitly stated by the people themselves; others are implicit in the consistency of their behavior under comparable conditions. There may be included descriptions of actual behavior, but these are generally illustrative of general customs. There may be specific genealogies, but these will be primarily samples to give substance to kinship practices and marriage customs. There may be detailed descriptions of a day's fishing activities, but these will serve primarily to point out customary fishing techniques and the structure of the work group. Even case studies of life histories are usually viewed as ways of providing specific instances of the ways in which culture and social structure manifest themselves in real life situations.

In addition to restricting the scope of his observations in this fashion it is inevitable that the anthropologist should be even further limited by his intellectual interests and biases. Many generalizations which he could make, many tribes which he might specify, he may fail to note because his orientation does not direct a search for them. If his own theoretical interests do not guide him and if the natives themselves do not explicitly emphasize some facet of their behavior, the anthropologist may fail to observe generalities that otherwise would be quite obvious and easily identified. I will return to this matter a little later on, for it is of especial interest to those concerned with the potential contributions of anthropology to psychology.

However limited they may be, the generalizations brought back from the field by anthropologists supply us with a vast fund of empirical observation concerning regularities in human behavior under a great variety of conditions. With all its defects, here exists a body of careful observations of human behavior that far exceeds anything even remotely comparable on any other animal species. In many instances the literature contains the results of large numbers of independent observations of the same people. Sometimes there are whole series of these observations fortunately so distributed in time that minute changes have been meticulously recorded.

Let us turn now for a moment to the work of the psychologists. In dealing with the subject matter of human social behavior the psychologist has labored under a very severe handicap. He has been denied, for the most part, one of the most powerful scientific tools now available to the student of natural phenomena. The explanation is obvious enough and scarcely deserves more than passing mention. People simply do not like to be experimented with except under very restricted and particular conditions. Public opinion certainly would not permit extensive social or cultural experimentation in our society regardless of the humanitarian aims which it might ultimately serve.

The psychologist, therefore, interested in human behavior has been forced to choose one of three paths which are essentially detours to the experimental investigation of his subject. One detour retains the experimental method and takes to rats, dogs, guinea pigs, monkeys, or apes, hoping to generalize therefrom to humankind. The great advantages of this approach are the demonstrable ones of controlled experiment. The disadvantages are equally clear. The subjects of these experiments are not human beings as animals nor are they members of a social group in anything like the sense that every person is a member of a society. Hypotheses concerning human behavior may be extrapolated from animal experimentation but before their acceptance they must be independently tested by reference to the behavior of people living under the condition of social life.

Another detour is that provided by the study of the maladjusted person, the mentally ill individual. Psychiatrists and psychoanalysts engaged in the attempt to cure neuroses and psychoses are provided with access into the mental and emotional life of individuals whose constitution and experience have combined to place them in a position requiring medical attention. This detour has the great advantage of dealing with socialized human beings. But it has many shortcomings. The persons examined are exceptional individuals, selected because of the peculiar symptoms they exhibit. Experimentation in the sense of the controlled manipulation of variables is impossible. Even more handicapping from the anthropological point of view is the fact that psychiatrists and psychoanalysts have had as patients people confined pretty much to our society, i.e., to the products of western European civilization.

Still another detour consists of utilizing tests of one sort or another which involve presenting different subjects with standard sets of stimuli. For the most part the uses of such tests have been confined to subjects brought up in western European society. When the attempt is made to apply such tests as the Rorschach or TAT to members of some quite different society, a number of problems arise that make difficult their interpretation. This is too lengthy a question to discuss within the confines of this paper. For the reader interested in examining the problems further, reference is made to the chapter by A. Irving Hallowell in *For a Science of Social Man,* The Macmillan Company, New York, 1954.

The detours around experimentation in the study of human social behavior have for the most part been ingenious and productive. They provide us with many tentative hypotheses concerning the nature of social life. But the problem remains of how to rigorously test these hypotheses by reference to actual human behavior and how to establish the applicability of these hypotheses as general principles governing human social life and culture throughout the world.

One way of proceeding with this objective is to capitalize upon the natural history data provided by the anthropologists. Here are a multitude of careful observations of how people behave under such varied conditions that they may be thought of as exhibits of natural experimentation. Some efforts have already been made in this direction. Outstanding examples of such comparative study are G. P. Murdock's *Social Structure,* J. W. M. Whiting and Irvin Child's *Child Training and Personality,* C. S. Ford and Frank Beach's *Patterns of Sexual Behavior.* Other similar studies are being currently pursued. But viewed broadly, relatively little attention has been paid by either the anthropologists or the psychologists to the possibility of rigorously examining seemingly fruitful ideas about human social life in the light of the accumulated cross-cultural evidence provided by existing ethnographies and studies of culture change.

There are many reasons for this relative lack of attention to the cross-cultural examination of hypotheses concerning human behavior and culture. The one that stands out perhaps more prominently than any other is that the mass of ethnographic data available for this purpose is so difficult to get at and to use. There are literally millions of volumes and articles containing information of relevance to the development of social science. But they are scattered in so many libraries and written down in so many different languages that to locate them and to find the specific items of interest in relation to a particular theoretical position looms a forbidding task. To check even a simple relationship for which the data are known to be available in the literature, presents such imposing problems of searching that it discourages even the most fanatical student of social life.

In 1937, at the Institute of Human Relations, Yale University, a major step toward remedying this situation was undertaken. Following in the tradition of Herbert Spencer and William Graham Sumner, who were perhaps the first to formulate the advisability of organizing our information about people in usable form, the attempt was made to devise a system which could eventually provide the social scientist with ready access to the data he needs. It was recognized at the outset that bibliographical aids and references would not be sufficient. Furthermore, abstracts of original sources could not be sufficiently precise and detailed because of the very nature of the material. For these reasons the sources themselves would have to be broken apart and reorganized.

The reorganization of our data on human societies required first of all a topical classification that could be used to order information on any society, from the most primitive to the most complex. This was ultimately achieved as the Outline of Cultural Materials. (Third revised edition, published by the Human Relations Area Files, Inc., 421 Humphrey Street, New Haven, Connecticut, 1949.) Using this topical outline, information on a selected number of societies was systematically examined and copied paragraph by paragraph. Each segment was assigned its appropriate topical allocations and placed in a vertical file, a duplicate for each different topic referred to in the paragraph. The result was a systematic file of original source material, translated into English where necessary, including the available data on the society selected for coverage. The researcher then looking for specific aspects of the society concerned had at his fingertips all the information that the literature could provide. The countless hours of searching through libraries and disorganized source materials were avoided. If files of this sort could be built for a large number of societies, cross-cultural research could begin in earnest.

A task as immense as this requires the cooperative effort of many people. A major step toward bringing about the necessary cooperation took place in 1949. With the assistance of an initial grant from the Carnegie Corporation, five major universities joined together in 1949 to form the Human Relations Area Files, Inc. Since that time the organization has grown to sixteen universities—University of Chicago, University of Colorado, Cornell University, Harvard University, University of Hawaii, Indiana University, State University of Iowa, University of Michigan, University of North Carolina, University of Oklahoma, University of Pennsylvania, Princeton University, University of Southern California, University of Utah, University of Washington, and Yale University—each of which contribute in money and in professional assistance to the development of organized files of information on the world's societies. Each university receives its own copy of the files as they are produced by the organization.

Even with the cooperation of sixteen universities the labor involved in organizing the extensive literature on the societies of the world is enormous and it will take many years before even a substantial sample of human cultures have been covered in this fashion. To date the HRAF has organized the available information on only some forty societies, but it is moving steadily ahead with ever increasing efficiency and support.

When a sufficient sample of human societies has been covered and the information available on them has been sifted and sorted and put in organized files then innumerable hypotheses concerning human behavior and culture can be subjected to initial cross-cultural test in an amazingly short time. (G. P. Murdock's article, "Family Stability in Non-European Cultures." To be sure, many hypotheses will demand information that is not available simply because the original researchers failed to observe the phenomenon in question. And this leads to the next and all-important step of educating the ethnographers who go into the field to collect the data relevant to the testing of current theoretical formulations. A search of the files will at least expose to the researcher what is available in the existing literature and what is not. It will permit a concerted attempt to fill in crucial gaps in our knowledge about particular peoples so that more adequate testing of our hypotheses becomes possible.

Briefly then, some of the potential contributions of anthropology to psychology and a general science of human social behavior are these. The presently available accumulation of ethnographic data, when organized, will provide a cross-cultural laboratory for the preliminary testing of hypotheses concerning human behavior. Armed with sharpened insights derived from psychological research and theory and with questions raised by the cross-cultural examinations of current theoretical formulations, anthropologists will go into the field to collect the new and relevant data needed for more refined analysis and test. As communication barriers between the two disciplines continue to break down and disappear the development of a truly general science of human behavior and social living will emerge—a new social psychology whose principles and theories will be everywhere applicable to the peoples of the world.

# THE SCIENCE OF LEARNING AND THE ART OF TEACHING

B. F. SKINNER

SOME promising advances have been made recently in the field of learning. Special techniques have been designed to arrange what are called "contingencies of reinforcement"—the relations which prevail between behavior on the one hand and the consequences of that behavior on the other—with the result that a much more effective control of behavior has been achieved. It has been argued long that an organism learns mainly by producing changes in its environment, but it is only recently that these changes have been carefully manipulated. In traditional devices for the study of learning—in the serial maze, for example, or in the T-maze, the problem box, or the familiar discrimination apparatus—the effects produced by the organism's behavior are left to many fluctuating circumstances. There is many a slip between the turn-to-the-right and the food-cup at the end of the alley.

It is not surprising that techniques of this sort have yielded only very rough data from which the uniformities demanded by an experimental science can be extracted only by averaging many cases. In none of this work has the behavior of the individual organism been predicted in more than a statistical sense. The learning processes which are the presumed object of such research are reached only through a series of inferences.

Current preoccupation with deductive systems reflects this state of the science.

Recent improvements in the conditions which control behavior in the field of learning are of two principal sorts. The Law of Effect has been taken seriously; we have made sure that effects *do* occur and that they occur under conditions which are optimal for producing the changes called learning. Once we have arranged the particular type of consequence called a reinforcement, our techniques permit us to shape up the behavior of an organism almost at will. It has become a routine exercise to demonstrate this in the classroom by conditioning such an organism as a pigeon. Simply by presenting food to a hungry pigeon at the right time, it is possible to shape up three or four well-defined responses in a single demonstration period—such responses as turning around, pacing the floor in the pattern of a figure eight, standing still in a corner of the demonstration apparatus, stretching the neck, or stamping the foot. Extremely complex performances may be reached through successive stages in the shaping process, the contingencies of reinforcement being changed progressively in the direction of the required behavior. The results are often quite dramatic. In such a demonstration one can *see* learning take place. A significant change in behavior is often obvious as the result of a single reinforcement.

A second important advance in technique permits us to maintain behavior in given states of strength for long periods of time. Reinforcements continue to be important, of course, long after an organism has learned *how* to do something, long after it has acquired behavior. They are necessary to maintain the behavior in strength. Of special interest is the effect of various schedules of intermittent reinforcement. Charles B. Ferster and the author are currently preparing an extensive report of a five-year research program, sponsored by the Office of Naval Research, in which most of the important types of schedules have been investigated and in which the effects of schedules in general have been reduced to a few principles. On the theoretical side we now have a fairly good idea of why a given schedule produces its appropriate performance. On the practical side we have learned how to maintain any given level of activity for daily periods limited only by the physical exhaustion of the organism and from day to day without substantial change throughout its life. Many of these effects would be traditionally assigned to the field of motivation, although the principal operation is simply the arrangement of contingencies of reinforcement.

These new methods of shaping behavior and of maintaining it in strength are a great improvement over the traditional practices of professional animal trainers, and it is not surprising that our laboratory results are already being applied to the production of performing animals for commercial purposes. In a more academic environment they have been used for demonstration purposes which extend far beyond an interest in learning as such. For example, it is not too difficult to arrange the complex contingencies which produce many types of social behavior. Competition is exemplified by two pigeons playing a modified game of Ping-pong. The pigeons drive the ball back and forth across a small table by pecking at it. When the ball gets by one pigeon, the other is reinforced. The task of constructing such a "social relation" is probably completely out of reach of the traditional animal trainer. It requires a carefully designed program of gradually changing contingencies and the skillful use of schedules to maintain the behavior in strength. Each pigeon is separately prepared for its part in the total performance, and the "social relation" is then arbitrarily constructed. The sequence of events leading up to this stable state are excellent material for the study of the factors important in nonsynthetic social behavior. It is instructive to consider how a similar series of contingencies could arise in the case of the human organism through the evolution of cultural patterns.

Cooperation can also be set up, perhaps more easily than competition. We have trained two pigeons to coordinate their behavior in a cooperative endeavor with a precision which equals that of the most skillful human dancers. In a more serious vein these techniques have permitted us to explore the complexities of the individual organism and to analyze some of the serial or coordinate behaviors involved in attention, problem solving, various types of self-control, and the subsidiary systems of responses within a single organism called "personalities." Some of these are exemplified in what we call multiple schedules of reinforcement. In general, a given schedule has an effect upon the rate at which a response is emitted. Changes in the rate from moment to moment show a pattern typical of the schedule. The pattern may be as simple as a constant rate of responding at a given value, it may be a gradually accelerating rate between certain extremes, it may be an abrupt change from not responding at all to a given stable high rate, and so on. It has been shown that the performance characteristic of a given schedule can be brought under the control of a particular stimulus and that different performances can be brought under the control of different stimuli in the same organism. At a recent meeting of the American Psychological Association, Dr. Ferster and the author demonstrated a pigeon whose behavior showed the pattern typical of "fixed-interval" reinforcement in the presence of one stimulus and, alternately, the pattern typical of the very different schedule called "fixed ratio" in the presence of a second stimulus. In the laboratory we have been able to obtain performances appropriate to *nine* different schedules in the presence of appropriate stimuli in random alternation. When Stimulus 1 is present, the pigeon executes the performance appropriate to Schedule 1. When Stimulus 2 is present, the pigeon executes the performance appropriate to Schedule 2, and so on. This result is gratifying because it makes the extrapolation of our laboratory results to daily life much more plausible. We are all constantly shifting from schedule to schedule as our immediate environment changes, but the dynamics of the control exercised by reinforcement remain essentially unchanged.

It is also possible to construct very complex *sequences* of schedules. It is not easy to describe these in a few words, but two or three examples may be mentioned. In one experiment the pigeon generates a performance appropriate to Schedule A where the reinforcement is simply the production of the stimulus characteristic of Schedule B, to which the pigeon then responds appropriately. Under a third stimulus, the bird yields a performance appropriate to Schedule C where the reinforcement in this case is simply the production of the stimulus characteristic of Schedule D, to which the bird then responds appropriately. In a special case, first investigated by L. B. Wyckoff, the organism responds to one stimulus where the reinforcement consists of the *clarification* of the stimulus controlling another response. The first response becomes, so to speak, an objective form of "paying attention" to the second stimulus. In one important version of this experiment we could say that the pigeon is telling us whether it is "paying attention" to the *shape* of a spot of light or to its *color*.

One of the most dramatic applications of these techniques has recently been made by Floyd Ratliff and Donald S. Blough, who have skillfully used multiple and serial schedules of reinforcement to study complex perceptual processes in the infrahuman organism. They have achieved a sort of psychophysics without verbal instruction. In a recent experiment by Blough, for example, a pigeon draws a detailed dark-adaptation curve showing the characteristic breaks of rod and cone vision. The curve is recorded continuously in a single experimental period and is quite comparable with the curves of human subjects. The pigeon behaves in a way which, in the human case, we would not hesitate to describe by saying that it adjusts a very faint patch of light until it can just be seen.

In all this work, the species of the organism has made surprisingly little difference. It is true that the organisms studied have all been vertebrates, but they still cover a wide range. Comparable results have been obtained with pigeons, rats, dogs, monkeys, human children, and most recently, in collaboration with Ogden R. Lindsley, human psychotic subjects. In spite of great phyllogenetic differences, all these organisms show amazingly similar properties of the learning process. It should be emphasized that this has been achieved by analyzing the effects of reinforcement and by designing techniques which manipulate reinforcement with considerable precision. Only in this way can the behavior of the individual organism be brought under such precise control. It is also important to note that through a gradual advance to complex interrelations among responses, the same degree of rigor is being extended to behavior which would usually be assigned to such fields as perception, thinking, and personality dynamics.

From this exciting prospect of an advancing science of learning, it is a great shock to turn to that branch of technology which is most directly concerned with the learning process—education. Let us consider, for example, the teaching of arithmetic in the lower grades. The school is concerned with imparting to the child a large number of responses of a special sort. The responses are all verbal. They consist of speaking and writing certain words, figures, and signs which, to put it roughly, refer to numbers and to arithmetic operations. The first task is to shape up these responses—to get the child to pronounce and to write responses correctly, but the principal task is to bring this behavior under many sorts of stimulus control. This is what happens when the child learns to count, to recite tables, to count while ticking off the items in an assemblage of objects, to respond to spoken or written numbers by saying "odd," "even," "prime," and so on. Over and above this elaborate repertoire of numerical behavior, most of which is often dismissed as the product of rote learning, the teaching of arithmetic looks forward to those complex serial arrangements of responses involved in original mathematical thinking. The child must acquire responses of transposing, clearing fractions, and so on, which modify the order or pattern of the original material so that the response called a solution is eventually made possible.

Now, how is this extremely complicated verbal repertoire set up? In the first place, what reinforcements are used? Fifty years ago the answer would have been clear. At that time educational control was still frankly aversive. The child read numbers, copied numbers, memorized tables, and performed operations upon numbers to escape the threat of the birch rod or cane. Some positive reinforcements were perhaps eventually derived from the increased efficiency of the child in the field of arithmetic and in rare cases some automatic reinforcement may have resulted from the sheer manipulation of the medium—from the solution of problems or the discovery of the intricacies of the number system. But for the immediate purposes of education the child acted to avoid or escape punishment. It was part of the reform movement known as progressive education to make the positive consequences more immediately effective, but anyone who visits the lower grades of the average school today will observe that a change has been made, not from aversive to positive control, but from one form of aversive stimulation to another. The child at his desk, filling in his workbook, is behaving primarily to escape from the threat of a series of minor aversive events – the teacher's displeasure, the criticism or ridicule of his classmates, an ignominious showing in a competition, low marks, a trip to the office "to be talked to" by the principal, or a word to the parent who may still resort to the birch rod. In this matter of aversive consequences, getting the right answer is in itself an insignificant event, any effect of which is lost amid the anxieties, the boredom, and the aggressions which are the inevitable by-products of aversive control. [Skinner, B. F. *Science and Human Behavior,* New York, 1953.]

Secondly, we have to ask how the contingencies of reinforcement are arranged. When is a numerical operation reinforced as "right"? Eventually, of course, the pupil may be able to check his own answers and achieve some sort of automatic reinforcement, but in the early stages the reinforcement of being right is usually accorded by the teacher. The contingencies she provides are far from optimal. It can easily be demonstrated that, unless explicit mediating behavior has been set up, the lapse of only a few seconds between response and reinforcement destroys most of the effect. In a typical classroom, nevertheless, long periods of time customarily elapse. The teacher may walk up and down the aisle, for example, while the class is working on a sheet of problems, pausing here and there to say right or wrong. Many seconds or minutes intervene between the child's response and the teacher's reinforcement. In many cases, for example, when papers are taken home to be corrected, as much as twenty-four hours may intervene. It is surprising that this system has any effect whatsoever.

A third notable shortcoming is the lack of a skillful program which moves forward through a series of progressive approximations to the final complex behavior desired. A long series of contingencies is necessary to bring the organism into the possession of mathematical behavior most efficiently. But the teacher is seldom able to reinforce at each step in such a series because she cannot deal with the pupil's responses one at a time. It is usually necessary to reinforce the behavior in blocks of responses—as in correcting a work sheet or page from a workbook. The responses within such a block must not be interrelated. The answer to one problem must not depend upon the answer to another. The number of stages through which one may progressively approach a complex pattern of behavior is therefore small, and the task therefore so much the more difficult. Even the most modern workbook in beginning arithmetic is far from exemplifying an efficient program for shaping up mathematical behavior.

Perhaps the most serious criticism of the current classroom is the relative infrequency of reinforcement. Since the pupil is usually dependent upon the teacher for being right, and since many pupils are usually dependent upon the same teacher, the total number of contingencies which may be arranged during, say, the first four years, is of the order of only a few thousand. But a very rough estimate suggests that efficient mathematical behavior at this level requires something of the order of 25,000 contingencies. We may suppose that even in the brighter student a given contingency must be arranged several times to place the behavior well in hand. The responses to be set up are not simply the various items in tables of addition, subtraction, multiplication, and division; we have also to consider the alternative forms in which each item may be stated. To the learning of such material we should add hundreds of responses concerned with factoring, identifying primes, memorizing series, using short-cut techniques of calculation, constructing and using geometric representations or number forms, and so on. Over and above all this, the whole mathematical repertoire must be brought under the control of concrete problems of considerable variety. Perhaps 50,000 contingencies is a more conservative estimate. In this frame of reference the daily assignment in arithmetic seems pitifully meager.

The result of all this is, of course, well known. Even our best schools are under criticism for their inefficiency in the teaching of drill subjects such as arithmetic. The condition in the average school is a matter of widespread national concern. Modern children simply do not learn arithmetic quickly or well. Nor is the result simply incompetence. The very subjects in which modern techniques are weakest are those in which failure is most conspicuous, and in the wake of an ever growing incompetence come the anxieties, uncertainties, and aggressions which in their turn present other problems to the school. Most pupils soon claim the asylum of not being "ready" for arithmetic at a given level or, eventually, of not having a mathematical mind. Such explanations are readily seized upon by defensive teachers and parents. Few pupils ever reach the stage at which automatic reinforcements follow as the natural consequences of mathematical behavior. On the contrary, the figures and symbols of mathematics have become standard emotional stimuli. The glimpse of a column of figures, not to say an algebraic symbol or an integral sign, is likely to set off—not mathematical behavior—but a reaction of anxiety, guilt, or fear.

The teacher is usually no happier about this than the pupil. Denied the opportunity to control via the birch rod, quite at sea as to the mode of operation of the few techniques at her disposal, she spends as little time as possible on drill subjects and eagerly subscribes to philosophies of education which emphasize material of greater inherent interest. A confession of weakness is her extraordinary concern lest the child be taught something unnecessary. The repertoire to be imparted is carefully reduced to an essential minimum. In the field of spelling, for example, a great deal of time and energy has gone into discovering just those words which the young child is going to use, as if it were a crime to waste one's educational power in teaching an unnecessary word. Eventually, weakness of technique emerges in the disguise of a reformulation of the aims of education. Skills are minimized in favor of vague achievements—educating for democracy, educating the whole child, educating for life. . . . And there the matter ends, for, unfortunately, these philosophies do not in turn suggest improvements in techniques. They offer little or no help in the design of better classroom practices.

There would be no point in urging these objections if improvement were impossible. But the advances which have recently been made in our control of the learning process suggest a thorough revision of classroom practices and, fortunately, they tell us how the revision can be brought about. This is not, of course, the first time that the results of an experimental science have been brought to bear upon the practical problems of education. The modern classroom does not, however, offer much evidence that research in the field of learning has been respected or used. This condition is no doubt partly due to the limitations of earlier research. But it has been encouraged by a too hasty conclusion that the laboratory study of learning is inherently limited because it cannot take into account the realities of the classroom. In the light of our increasing knowledge of the learning process we should, instead, insist upon dealing with those realities and facing a substantial change in them. Education is perhaps the most important branch of scientific technology. It deeply affects the lives of all of us. We can no longer allow the exigencies of a practical situation to suppress the tremendous improvements which are within reach. The practical situation must be changed.

There are certain questions which have to be answered in turning to the study of any new organism. What behavior is to be set up? What reinforcers are at hand? What responses are available in embarking upon a program of progressive approximation which will lead to the final form of the behavior? How can reinforcements be most efficiently scheduled to maintain the behavior in strength? These questions are all relevant in considering the problem of the child in the lower grades.

In the first place, what reinforcements are available? What does the school have in its possession which will reinforce a child? We may look first to the material to be learned, for it is possible that this will provide considerable automatic reinforcement. Children play for hours with mechanical toys, paints, scissors and paper, noisemakers, puzzles—in short with almost anything which feeds back significant changes in the environment and is reasonably free of aversive properties. The sheer control of nature is itself reinforcing. This effect is not evident in the modern school because it is masked by the emotional responses generated by aversive control. It is true that automatic reinforcement from the manipulation of the environment is probably only a mild reinforcer and may need to be carefully husbanded, but one of the most striking principles to emerge from recent research is that the *net* amount of reinforcement is of little significance. A very slight reinforcement may be tremendously effective in controlling behavior if it is wisely used.

If the natural reinforcement inherent in the subject matter is not enough, other reinforcers must be employed. Even in school the child is occasionally permitted to do "what he wants to do," and access to reinforcements of many sorts may be made contingent upon the more immediate consequences of the behavior to be established. Those who advocate competition as a useful social motive may wish to use the reinforcements which follow from excelling others, although there is the difficulty that in this case the reinforcement of one child is necessarily aversive to another. Next in order we might place the good will and affection of the teacher, and only when that has failed need we turn to the use of aversive stimulation.

In the second place, how are these reinforcements to be made contingent upon the desired behavior? There are two considerations here—the gradual elaboration of extremely complex patterns of behavior and the maintenance of the behavior in strength at each stage. The whole process of becoming competent in any field must be divided into a very large number of very small steps, and reinforcement must be contingent upon the accomplishment of each step. This solution to the problem of creating a complex repertoire of behavior also solves the problem of maintaining the behavior in strength. We could, of course, resort to the techniques of scheduling already developed in the study of other organisms but in the present state of our knowledge of educational practices, scheduling appears to be most effectively arranged through the design of the material to be learned. By making each successive step as small as possible, the frequency of reinforcement can be raised to a maximum, while the possibly aversive consequences of being wrong are reduced to a minimum. Other ways of designing material would yield other programs of reinforcement. Any supplementary reinforcement would probably have to be scheduled in the more traditional way.

These requirements are not excessive, but they are probably incompatible with the current realities of the classroom. In the experimental study of learning it has been found that the contingencies of reinforcement which are most efficient in controlling the organism cannot be arranged through the personal mediation of the experimenter. An organism is affected by subtle details of contingencies which are beyond the capacity of the human organism to arrange. Mechanical and electrical devices must be used. Mechanical help is also demanded by the sheer number of contingencies which may be used efficiently in a single experimental session. We have recorded many millions of responses from a single organism during thousands of experimental hours. Personal arrangement of the contingencies and personal observation of the results are quite unthinkable. Now, the human organism is, if anything, more sensitive to precise contingencies than the other organisms we have studied. We have every reason to expect, therefore, that the most effective control of human learning will require instrumental aid. The simple fact is that, as a mere reinforcing mechanism, the teacher is out of date. This would be true even if a single teacher devoted all her time to a single child, but her inadequacy is multiplied manyfold when she must serve as a reinforcing device to many children at once. If the teacher is to take advantage of recent advances in the study of learning, she must have the help of mechanical devices.

The technical problem of providing the necessary instrumental aid is not particularly difficult. There are many ways in which the necessary contingencies may be arranged, either mechanically or electrically. An inexpensive device which solves most of the principal problems has already been constructed. It is still in the experimental stage, but a description will suggest the kind of instrument which seems to be required. The device consists of a small box about the size of a small record player. On the top surface is a window through which a question or problem printed on a paper tape may be seen. The child answers the question by moving one or more sliders upon which the digits 0 through 9 are printed. The answer appears in square holes punched in the paper upon which the question is printed. When the answer has been set, the child turns a knob. The operation is as simple as adjusting a television set. If the answer is right, the knob turns freely and can be made to ring a bell or provide some other conditioned reinforcement. If the answer is wrong, the knob will not turn. A counter may be added to tally wrong answers. The knob must then be reversed slightly and a second attempt at a right answer made. (Unlike the flash card, the device reports a wrong answer without giving the right answer.) When the answer is right, a further turn of the knob engages a clutch which moves the next problem into place in the window. This movement cannot be completed, however, until the sliders have been returned to zero.

The important features of the device are these: Reinforcement for the right answer is immediate. The mere manipulation of the device will probably be reinforcing enough to keep the average pupil at work for a suitable period each day, provided traces of earlier aversive control can be wiped out. A teacher may supervise an entire class at work on such devices at the same time, yet each child may progress at his own rate, completing as many problems as possible within the class period. If forced to be away from school, he may return to pick up where he left off. The gifted child will advance rapidly, but can be kept from getting too far ahead either by being excused from arithmetic for a time or by being given special sets of problems which take him into some of the interesting bypaths of mathematics.

The device makes it possible to present carefully designed material in which one problem can depend upon the answer to the preceding and where, therefore, the most efficient progress to an eventually complex repertoire can be made. Provision has been made in the device for recording the commonest mistakes so that the tapes can be modified as experience dictates. Additional steps can be inserted where pupils tend to have trouble, and ultimately the material will reach a point at which the answers of the average child will almost always be right.

If the material itself proves not to be sufficiently reinforcing, other reinforcers in the possession of the teacher or school may be made contingent upon the operation of the device or upon progress through a series of problems. Supplemental reinforcement would not sacrifice the advantages gained from immediate reinforcement and from the possibility of constructing an optimal series of steps which approach the complex repertoire of mathematical behavior most efficiently.

A similar device in which the sliders carry the letters of the alphabet has been designed to teach spelling. In addition to the advantages which can be gained from precise reinforcement and careful programming, the device will teach reading at the same time. It can also be used to establish the large and important repertoire of verbal relationships encountered in logic and science. In short, it can teach verbal thinking. As to content instruction, the device can be operated as a multiple-choice self-rater.

Some objections to the use of such devices in the classroom can easily be foreseen. The cry will be raised that the child is being treated as a mere animal and that an essentially human intellectual achievement is being analyzed in unduly mechanistic terms. Mathematical behavior is usually regarded, not as a repertoire of responses involving numbers and numerical operations, but as evidences of mathematical ability or the exercise of the power of reason. It is true that the techniques which are emerging from the experimental study of learning are not designed to "develop the mind" or to further some vague "understanding" of mathematical relationships. They are designed, on the contrary, to establish the very behaviors which are taken to be the evidences of such mental states or processes. This is only a special case of the general change which is under way in the interpretation of human affairs. An advancing science continues to offer more and more convincing alternatives to traditional formulations. The behavior in terms of which human thinking must eventually be defined is worth treating in its own right as the substantial goal of education.

Of course the teacher has a more important function than to say right or wrong. The changes proposed would free her for the effective exercise of that function. Marking a set of papers in arithmetic—"Yes, nine and six *are* fifteen; no, nine and seven *are not* eighteen"—is beneath the dignity of any intelligent individual. There is more important work to be done —in which the teacher's relations to the pupil cannot be duplicated by a mechanical device. Instrumental help would merely improve these relations. One might say that the main trouble with education in the lower grades today is that the child is obviously not competent and *knows it* and that the teacher is unable to do anything about it and *knows that, too*. If the advances which have recently been made in our control of behavior can give the child a genuine competence in reading, writing, spelling, and arithmetic, then the teacher may begin to function, not in lieu of a cheap machine, but through intellectual, cultural, and emotional contacts of that distinctive sort which testify to her status as a human being.

Another possible objection is that mechanized instruction will mean technological unemployment. We need not worry about this at least until there are enough teachers to go around and until the hours and energy demanded of the teacher are comparable to those in other fields of employment. Mechanical devices will eliminate the more tiresome labors of the teacher, but they will not necessarily shorten the time during which she remains in contact with the pupil.

A more practical objection: Can we afford to mechanize our schools? The answer is clearly yes. The device I have just described could be produced as cheaply as a small radio or phonograph. There would need to be far fewer devices than pupils, for they could be used in rotation. But even if we suppose that the instrument eventually found to be most effective would cost several hundred dollars and that large numbers of them would be required, our economy should be able to stand the strain. Once we have accepted the possibility and the necessity of mechanical help in the classroom, the economic problem can easily be surmounted. There is no reason why the schoolroom should be any less mechanized than, for example, the kitchen. A country which annually produces millions of refrigerators, dishwashers, automatic washing machines, automatic clothes driers, and automatic garbage disposers can certainly afford the equipment necessary to educate its citizens to high standards of competence in the most effective way.

There is a simple job to be done. The task can be stated in concrete terms. The necessary techniques are known. The equipment needed can easily be provided. Nothing stands in the way but cultural inertia. But what is more characteristic of America than an unwillingness to accept the traditional as inevitable? We are on the threshold of an exciting and revolutionary period, in which the scientific study of man will be put to work in man's best interests. Education must play its part. It must accept the fact that a sweeping revision of educational practices is possible and inevitable. When it has done this, we may look forward with confidence to a school system which is aware of the nature of its tasks, secure in its methods, and generously supported by the informed and effective citizens whom education itself will create.

# REDUCTION

GUSTAV BERGMANN

TO examine the relations among the behavior sciences is a huge enterprise. The questions one may reasonably ask under this head are not only many, they are of different kinds. Some are practical. These raise points of strategy or policy. Strategy decides which research project shall be undertaken first and by whom, that is, by which specialty or combination of specialties. Policy creates and administrates the institutions within which strategy decisions are made. Some other questions are historical. These explore origins, growth, and mutual influences. Thus the record of the past, strategies, policies, and their outcome, is established. Scanning it, together with whatever else proves relevant, for patterns of cause and effect, one comes upon questions of a third kind, those of the science of science. Systematically speaking, this science is but one among the behavior sciences, just as its use of the historical record is but one instance of what behavior science always ought to do with it, whether it be the record of science, or of art, or of political institutions. There is still a further kind of question. This kind I just exemplified twice; once when I insisted that *systematically* considered the science of science is itself a behavior science; once when by the use of "ought" I implied that it illustrates the *proper* scientific use of history. Instead of "systematically" I could have said *logically;* instead of "proper," *logically proper.* There are then the logical questions that arise in an examination of the relations among the behavior sciences, not in the narrower sense of deductive logic but in that broader sense in which one now speaks of the logic or philosophy of science. Again, the fourfold distinction between practical, historical, scientific, and logical questions *about* science belongs itself to the logic of science, as does the distinction between any of these four on the one hand and questions *within* science on the other. This provides us with another hint as to what the philosophy of science is and what it tries to accomplish.

Nontechnically, philosophy means wisdom. But wisdom is no one's privilege. Nor is it a specialty or technical field of competence. The philosophy of science is. Thus, being a technical philosopher, I propose to discuss a topic in the philosophy of science. The one I chose is reduction. Though not the whole story, it is, as will transpire, very central to the logic of the relations among the behavior sciences. Accordingly, a good deal has been written about it during the past twenty or thirty years. I myself have taken part in the debate; so has, quite recently, one of my younger collaborators: [M. Brodbeck, "On the philosophy of the social sciences," *Philosophy of Science,* 21, 1954.

The literature is very large and spread over many disciplines. Also, it is sufficiently repetitious to make the task of selection onerous. For an adequate key to the literature, see the bibliography H. Feigl and M. Brodbeck have compiled in their recent anthology, *Readings in the Philosophy of Science,* Appleton-Century-Crofts, 1953.]

Most of what I shall say is therefore either not new or not my own. All I hope to do is to add or polish a point or to provide a new perspective here and there. Nor shall I try to explain in general terms what the philosophy of science is. If they are to stand up under close scrutiny, such explanations are both difficult and long-winded. Worse than that, a philosopher presenting them to a nonphilosophical audience takes the high-road to expository failure. I offer instead the following analysis as a sample.

Reduction means that something is being put into a certain relation to something else. One may ask, for instance, whether such group sciences as economics, anthropology, and sociology have been, shall be, or can be reduced to psychology; or whether psychology is in turn reducible to physiology. These are indeed questions raised whenever the relations among the behavior sciences are examined. Eventually I shall say something about them. But that will take me some time, for it requires some preparation. One obvious reason is this. We may think we know what it means to reduce one area or its science to another. Yet the idea is vague. Strictly speaking, only a *theory* can be reduced to another theory. An area, or more accurately, the science of it, is characterized by the concepts or terms, aside from the ubiquitous logical terms, that occur in its propositions. A theory is more than a set of terms or even of propositions. Hence, the difference between the clear idea and the one that is vague. Only after one has explained what it means for one or both of the two theories involved to be *comprehensive,* can the vague idea be given an explication. So I must first state, however succinctly, what a theory is and what it means for one to be comprehensive. But first of all a cautionary remark to forestall some tiresome misunderstandings.

Whatever the explications may be in detail, we all know that in any reasonable sense of the terms none of the behavior sciences has at present much of a theory, let alone one that could plausibly claim comprehensiveness. Thus one may wonder what point there is to our enterprise. Some may even fear that by engaging in it we commit ourselves logically to the existence, now or in the future, of such theories; and that practically, therefore, we favor even sterile theorizing over honest fact finding. We do nothing of the sort. What we do is entirely hypothetical, an analysis of meanings and logical possibilities. That this sort of thing is of some use I shall of course not prove. That, I hope, will show itself.

The oldest and perhaps the most familiar of all theories is Euclidean geometry. Its terms are "point," "straight line," "plane," "intersect," "distance," "circle," "cube," "cone," and so on. Like the terms of any theory, they fall into two classes. One contains only a few terms, called *undefined* or basic. With their referents we are familiar. The other contains all and only those terms that can be *defined* in terms of the basic ones. Hence we know their referents, too, in the sense that if we encounter one we recognize it. As a rule the division among terms can be made in more than one way. This is just one of the many subtleties I must neglect. The propositions of geometry, like those of any other theory, are either individual propositions describing individual configurations, however complex; or they are law statements. The latter fall into two classes. (This division, too, is as a rule not uniquely determined.) One contains a relatively small number of laws, called *axioms*. The other contains all and only those further law statements, called *theorems*, that follow deductively from the axioms. The most significant thing a theory can do is this. Given certain information about any configuration, *C*, it yields deductively a complete description of *C*. Failure of this description to agree with further information about *C* constitutes failure of the theory. This does not mean that *C* must be mentioned in any of the axioms. The configurations they mention are few and very simple. Nor need it be mentioned in any of the theorems we happen to have ready at hand, as it were. One can always deduce the theorem that fits *C*. This is the compositional feature of all theories. Its close analysis is well worth while. But I cannot go into that on this occasion.

Beginning with geometry as I did helps to introduce the main ideas in a natural order. The theories all other sciences strive for and have been striving for since the Galileo-Newtonian revolution all share a feature which geometry lacks. This feature is time. The configurations *C* on which modern science concentrates are all *processes*. What in geometry I rather vaguely called "certain information about *C*" becomes the description $C_o$ of a momentary state of a *system*. In conjunction with suitable theorems $C_o$ is expected to yield deductively the description $C_t$ of any earlier or later state of the system. To emphasize the pattern one may well say that science looks for process theories. The two words "system" and "process," formidable as they sound, cover no unexamined assumptions, philosophical or otherwise. If one has space enough, as I don't, one can say everything I just said without using them and introduce them afterwards to be used conveniently in this context. Outside of it they have no precise meaning. To speak of the momentary state of a system is not quite so harmless. The phrase suggests the idea of mathematical moments in analogy to mathematical points. The suggestion is strengthened by the long habit of the mathematical apparatus of real numbers which physics so successfully uses in dealing with processes. Yet there are no such things as mathematical moments or, for that matter, points. My statement could therefore do with some refinement in this respect. But I must not tarry. At any rate, the idea of process can be so explicated that it covers patterns which are mathematically much less sophisticated, such as, say, contemporary learning theory. Every process theory claims implicitly that it is *closed*. This means that the referents of its terms interact among themselves and with no others. It follows that if two systems, each of which is left to itself, are at one moment equal with respect to all occurrences of all the variables of the theory, then they have always been and will always be thus equal, provided of course the theory is successful. The phrase being left to itself covers another, purely spatial idea of closure. It, too, deserves analysis. But since I shall not need it here, I shall ignore it. Its common-sense core is clear enough.

An area is characterized by its terms or, as scientists would rather say, its variables. The notion is not completely precise. One cannot either enumerate or otherwise single out this class of terms in advance. The terms of a theory, on the other hand, are fixed. They belong either to the relatively small number of undefined terms we actually enumerate or they are definable by means of them. The notion of a theory being comprehensive of an area necessarily partakes of the vagueness. Only, this vagueness is as harmless as it is natural and ineradicable. A theory, then, is comprehensive of an area if it fulfills two conditions. 1. All variables of the area are definable in terms of the undefined variables of the theory. 2. All laws, whether known or yet to be discovered, which contain only the variables of the area are theorems of the theory. Obviously, one can never be sure that a theory is really comprehensive. This is exactly as one would expect it to be. A clear notion of comprehensiveness is no less useful for it.

Let me take time out from this game of explications to throw a first glance at the facts. *With respect to closure,* we saw that every theory must try to achieve it. That is part of its logic. Nor is there anything logically repugnant in the idea of a world which is dual or plural in the strange and unusual sense that it can be comprehensively explained by two or several theories with mutually exclusive sets of terms. The experiences, though not the explanation, of a split personality approach such a universe. In our world, to the best of our knowledge, no theory not literally comprehensive of everything is likely to be literally closed. Practically, closure is a matter of degree. In the behavior field the variables characteristic of each area interact of course with those of the physical environment. More significantly, it is by now also a truism that the variables of the several traditional areas such as economics, or sociology, or what have you, interact pervasively with each other. The notorious misfortunes of classical economics are a case in point. The trouble was that it insisted on the closure of its theory though it disregarded the variables economists now call institutional. In a discussion like this it may therefore be wise to abandon the traditional divisions, speaking instead, as I shall, simply of behavior science. Of this, more presently. *With respect to comprehensiveness,* only in physics, more precisely in physics-chemistry, could comprehensiveness plausibly be claimed for the several theories that succeeded each other during the past century or so. Yet they were several and they succeeded each other. The trees of theory do not grow into the sky. On the other hand, considering the density of the causal nexus in our world, one may well wonder whether a theory really comprehensive of an area must not in some sense be part of one that comprehends all areas. These are the reflections that lead to the idea of reduction. So I turn finally to its examination.

Historically, four cases stand out, one through success, the others through the attention they attracted or still attract. Let me call them the *four classical cases.* The first is the reduction of gas theory and whatever else goes with it in this context to mechanics. This is the one instance I know in which both theories could at the time plausibly claim comprehensiveness. Moreover, its success was very substantial. It was indeed a triumph, though eventually there came, as always, the hour of truth. The second classical case is the reduction of physiology to physics-chemistry. In its day it shook the academic halls. By now it has fallen into desuetude, the reason being that the physiologists themselves state their theories to an ever increasing extent in physicochemical terms. And reduction, we remember, is a matter of theory. Thus, if I may exaggerate for the sake of structure, there is not much left to be reduced in this instance. The third and fourth cases, both still very much alive, are the reduction of psychology to physiology and of social science to psychology.

Next, for a pair of words and an assumption. The reducing theory is often spoken of as the mikrotheory, the reduced one as the makrotheory. The labels come from the first classical case. Like all labels, they have puzzled some and started some others on wrong ideas. Yet they are convenient; so I shall adopt them without further ado. The assumption is that the makro- and the mikrotheory have no undefined terms and no axioms in common. This assumption is really quite unrealistic. Geometry, for instance, is all-pervasive, and any behavior theory must surely contain physical and biological parameters. Yet the assumption is both useful and justifiable. The advantage is that it permits us to dispense with some qualifications; the justification is that these qualifications, though wordy, are rather obvious. I am ready to define reduction. The definition itself is brief; the comments it requires are not.

A theory has been reduced to another if and only if two conditions are fulfilled. 1. A correspondence has been established which coordinates to each undefined term of the makrotheory, and therefore derivatively to all its terms, a defined term of the mikrotheory. Metaphorically, the coordination may be spoken of as a translation. 2. Thus translated, every makroaxiom, and therefore derivatively every makrotheorem, becomes a mikrotheorem (not a mikroaxiom).

Notice that the first clause does not really conflict with the familiar idea that one makrostate may correspond to many mikro*states*. Logically this means merely that the coordinated mikro*term* is very complex, a disjunction of many alternatives in the sense in which "being green or blue" is a disjunction of two. Nor is there any trouble because the simultaneous translation of several terms may for each of them limit its respective alternatives. This amounts merely to further clauses in the fully expanded translation rule. One would not reasonably expect this rule to be very simple. This, by the way, takes care of one of the interaction features about which the Gestaltists always worry so much.

Notice next the studied abstractness of "correspondence" and the insistence that "translation" is used metaphorically. What this abstractness and this metaphor conveniently signify is the purely formal similarity between *three materially different situations.* The first is that of classical atomistics. Remember that a theory is materially a theory and not merely a verbal structure only if we are familiar with the referents of its basic terms. We were not then and we are not now in a relevant sense familiar with the basic entities of the so-called model. The correspondence is therefore *made,* not discovered. What is discovered is, rather, its usefulness. By putting it so bluntly, perhaps even a bit too bluntly, I do not intend to reawaken the sleeping dogs of fictionalism and realism. The trees they would bark up do not in any case stand in the garden of science. The second situation is that of the second and third classical cases, physiology to physics-chemistry and psychology to physiology. We are familiar with organic as well as with inorganic objects and with items of behavior as well as with the insides of the bodies that display them. In the second situation the correspondences are therefore *discovered*. What one there discovers are laws and not, absurdly, translation rules. If one wishes he may of course deny that there can be such laws. Such a one denies that, even with the precautions of the last paragraph, a behavioral item or state corresponds uniquely to a physiological item or state. The business of pointing out the absurdities this implies is best left to the epistemological seminar. But perhaps I had better mention that for a psychologist who knows what he is about, a mental content is also a behavioral state. What that neglects, and it does neglect something, belongs again to epistemology and not to either science or its philosophy. To repeat, in this situation, the correspondence is a matter of discovered or discoverable tangible and unproblematic fact. One may of course have made an unsuccessful induction. That possibility is always present, so one need hardly ever mention it. The third situation arises in the reduction of social science to psychology. Thus we must first ask what the terms of the two areas are. It is high time anyway that we settle this question. The terms of psychology mention items of individual behavior. Those of social science, I like to call them *group terms*, are exemplified by, say, "average income," "rate of capital accumulation," "the morale of the Army," "the ideology of the Church." After a fashion we know what they mean. Sometimes we probably make sense when we use them, sometimes we very probably don't. More significantly, they are of two kinds. Some are obviously definable in psychological (and, of course, as always, environmental) terms; at least schematic definitions are actually available for many among them. Let me call these the *statistical* group terms, borrowing the name from the manner in which many of them are defined. For the other kind, such definitions are at the moment not available; some doubt whether they will ever come forth. These terms I like to call the *institutional* group terms. "Holistic," I think, would also convey the idea. The sample I gave contains both kinds. At this point I confess to a bias. I hold that the only way to provide any group term with unproblematic meaning is to define it in psychological terms. Some may think this is just common sense. So do I. But since I hold this position also on philosophical grounds, I had better call it on this occasion a bias. If one accepts it—as I shall, because I must, since I do not understand any other — then the correspondence between the two theories rests on *definition*. This is the third situation covered by the abstract idea of correspondence and the metaphor of translation.

That much for the first clause in the explication of reduction. I turn to the second.

There are only two alternatives. Either the two theories are about equally well developed, that is, of about equal scope and reliability, or the mikrotheory is much better developed. For we would of course never attempt to reduce the "better" theory to one that is "poorer." These, however, are very vague words; I must hurry to get rid of them. Schematically, then, I represent the rough idea of a well-developed theory by the assumption that it is comprehensive of its area. The notion of a theory being less well developed than its prospective mikrotheory I represent by the assumption that it is not really in its area a closed process theory but merely incomplete knowledge. Of course, I shall have to explain what I mean by the phrase incomplete knowledge. Then we shall have *two main patterns:* 1. the reduction of one comprehensive theory to another; 2. the reduction of incomplete knowledge to a comprehensive theory. The discussion of the first main pattern is not difficult. So I shall tackle it next. But let us first see how the four classical cases distribute themselves. That the reduction to the mechanical model came reasonably close to the first pattern, I mentioned before. The second classical case I skip for the moment since it is, as we saw, a little out of date. As for the last two, some feel, not entirely without grounds, that physiology is on the whole, though not necessarily in the area most crucial to reduction, so much better developed than psychology that the state of their common affairs approximates the second schema. Some others feel, again not entirely without grounds, that the same thing is true of the relation between psychology and social science. The intrepid pioneer of the latter view at the turn of the century was William McDougall, who led the revolt against Duerckheim. Behind Duerckheim looms Hegel.

Now for the first main pattern, one comprehensive theory reduced to another. In the third situation, correspondence by definition, the makrotheory is simply a deductive consequence of the mikrotheory and there is nothing more to be said. In the second situation, correspondence by laws, the makrotheory is a deductive consequence of the mikrotheory in conjunction with these laws. In the first situation we tinker with the correspondence until we get what we want. In the case of the mechanical model, which is really the only substantial instance, we have now for quite some time also tinkered with the mikrotheory itself; and that, as every physicist knows, is putting it mildly. This goes to show the peculiarity of a situation where the mikrotheory by itself is in a sense merely a verbal structure. I return for a moment to the second situation, since it is the proper occasion for a remark long overdue. Physics-chemistry and physiological theory still quaintly stated in exclusively physiological terms are a fair approximation to it. Assume now that one physiological axiom translates, not into a theorm of physics-chemistry, but into one of its axioms. Loosely speaking, that amounts to this. We find that we can "reduce" physiology to physics-chemistry provided we take at least one elementary physiological process, say, some process in the living cell, state it in physicochemical terms and adjoin this statement as a further axiom to what we ordinarily call physics and chemistry. We would not feel that we have really "reduced" anything. This is why the definition of reduction requires all makroaxioms to translate into mikrotheorems, not into mikroaxioms.

A closed process theory, and I add the redundant "closed" merely for emphasis, is in an obvious sense complete knowledge about its variables. Unless one craves to "reduce" them, in some sense of "reduce," to some others, it is hard to imagine what more one could possibly want to know about them in a scientific way. This is why I propose to call *incomplete knowledge* all sorts of laws obtaining among the variables of a process theory as long as, taken jointly, they are not equivalent to the theory itself. In other words, each such law follows deductively from the theory, but the theory does not deductively follow from their conjunction. This also makes historical sense. Before one can venture sanely to construct a theory, incomplete knowledge about its variables must have accumulated. Theories do not fall from the sky. Let me put a little flesh on these logical bones. A law may state that if a configuration has at one time one feature, it will at some later time, or at all later times, or after a certain lapse of time have some other feature. Obviously, such a law says much less than the process theory which implies it. Yet it may be very reliable. For instance, one or two things about the future states of a man who has been properly hanged by the neck have been known quite reliably for quite a long time. If a name is wanted, this kind of incomplete knowledge may well be called knowledge of *typical connections*. It is at the root of the cause-effect terminology which is so deeply embedded in our language. Again, we may have *statistical* knowledge, that is, in its crudest form, we know that certain features produce certain others later on in most instances. If we had the process theory, we could deduce these frequencies from it in conjunction with observed frequencies of kinds or ranges of initial conditions. And so on, and so on. I have no ambition to produce an exhaustive classification of all sorts of incomplete knowledge. One more sort, though, deserves mention because of its significance in the behavior field and because it anticipates the logical structure of a full-fledged process theory. Laws of this sort, I call them *developmental laws,* assert that if a system is in a certain state now, then it was in a certain other state some time ago, will be in a third one some time from now, in a fourth some time after that, and so on. What is known, in other words, is a typical succession of stages, sometimes quite a few. As a rule, the time intervals are rather indeterminate, the states quite broadly defined, and there is also either explicitly or implicitly the clause, "in the ordinary course of events" —all unmistakable marks of incomplete knowledge. Comte, Spencer, Marx, the proponents of so-called parallelism among the anthropologists, and many others claimed to have discovered developmental laws. We do undoubtedly have some such knowledge not only in biology, where by now we can often do much better, but also, at least of a rough kind, in the psychological field of personality. Laws stating trends are instances in economics. The first and second principles of thermodynamics are in fact laws of this sort. But they are of course not the whole of physical theory.

I am ready to turn to the second main pattern, comprehensive mikrotheory, incomplete knowledge in the makroarea.

Here a new question arises or, if you please, the reduction issue must be reworded; otherwise, there is nothing of interest left. For, as things stand by assumption, there is nothing, or only very little, to be reduced. But one may ask what chances there are, logically speaking, for there ever being a comprehensive makrotheory. Failing this, what are the chances for a substantial makrotheory, a theory being called substantial if it handles a goodly number of variables with practically acceptable closure. There is no general answer. The answer depends in each case on the specific structure of the mikrotheory. Take the definitional situation. The question is whether a set of nontrivial definitions for makroterms can be devised so that when two mikrostates agree in their makrodescription at one time, the mikroprocess will so transform them that their makrodescriptions will agree at all times. Clearly, that depends on what the mikrolaws specifically are.

Let me once more interrupt the serene course of analysis, this time for a digression of a very different sort. Up to this point I have studiously abstained from fetching illustrations lest I seem to curry favor for what I believe is worth while in itself. Now I shall break this self-imposed rule. The case I just discussed abstractly fits exactly what has for so long been debated as the role of the hero in history, more soberly, the role of the individual, or of some individuals, in the social process. If I am not mistaken, very many of our contemporaries take it for granted that there is a comprehensive or at least a very substantial theory in the group variables of social science. All we need to do is formulate it. Nor is this supreme confidence limited to one side of the great curtain that divides our world. It is truly part of the *Zeitgeist*. Considering the actual lack or poverty of present social theory I find this amazing. I also find it appalling. The source from which the faith springs is patently ideological. What poisons it is collectivism. Calling the collectivistic ideology a poison I have of course shown my hand, that is, I have made a value judgment. So I shall also say this. As an analyst, I undertake to uncover the logical flaw, or the factual ignorance, or the hidden ideological premise, or any combination of these three, in any argument that claims to prove from our present knowledge that a comprehensive or even a substantial theory in social science is either logically or factually impossible. We do not know enough either one way or the other. That is just the point.

I return to analysis. I wish to make it as clear as I can that I am not advocating an extreme reductionism, whatever that may mean. In the social science area, it seems to me, for whatever a layman's impressions are worth, that at least the economists have in this century developed a theory sufficiently complex to be logically interesting as well as not wholly unsuccessful in its predictions within some range of the institutional variables. In psychology I am quite vocally on record in favor of the so-called behavior theory, which is certainly a makrostructure with respect to physiology. This is a practical judgment. Here, too, we are faced with a theory of some logical interest, making at least statistical predictions about individual behavior which can be and are being tested in the laboratory; while the physiological psychologists have at this point nothing they could tell us about the outcome of such experiments. Yet there are some who, because of their enthusiasm for psychophysiology, reject behavior theory. If what they say were merely that, measured against an eventual comprehensive physiological theory all knowledge stated in behavior terms will probably prove woefully incomplete, I would have no quarrel with them. Nor am I bothered either by their research preference or by what I take to be their conviction that even in the absence of a physiological theory a psychological one, such as it is, is not worth having. One would think that in science, too, one bird in hand is better than two in the bush. But again, since I am not really a psychologist, I am not really bothered. What does bother me, as a philosophical analyst, is the quality of the arguments which we hear again and again from the reductionist camp against behavior theory and the experimentation it stimulates. And, to repeat, wherever and whenever theory in social science shows tangible results, I am equally prepared to defend it against ideological or dogmatic demands for psychologizing. The logic of the two situations is the same. They are both approximations, though very rough ones indeed and of different degrees of roughness, to what I called the second main pattern.

In conclusion, I should like to introduce into this analysis a distinction that has interested me for some time. It is a distinction between two kinds of process, or more accurately, a generalization of the notion of process as I explicated it. According to this explication, a process theory permits inference from a momentary state $C_o$ of a system to any of its later or earlier states $C_t$. Such theories I call now *systematic* process theories. A historical law provides for some inference about a later state $C_t$, not from $C_o$ alone, but from $C_o$ in conjunction with additional information about an earlier state or states. A theory which contains among its axioms at least one historical law I call a *historical* process theory. Since there are literally no moments, it is in a sense merely a distinction of degree. Practically, it is striking. To grasp it firmly, consider the difference between historical and developmental laws. Take three successive events or stages, *A, B, C*. The schema of a developmental law connecting them is, "If *A* now, then *B* later and still later *C"*; or, with the inference based on *B*, "If *B* now, then *A* earlier and *C* later"; similarly for the inference based on *C*. The historical schema connecting the three items reads, "If *B* now *and A* earlier, then *C* later" or, equivalently, "If *A* earlier, then *if B* now *then C* later." The second if-then clause marks the difference.

Let us check how the several areas line up on this criterion. Physicochemical and physiological theory are of course systematic. Nor do I see any straw in the winds of scientific opinion to indicate that this would change in the foreseeable future. Many psychological laws are indubitably historical. The classical British Associationists were the first to bring that out, though they did not state it explicitly. More recently, Freud has brought it home to all of us with epochal impact. The sketches of laws the psychoanalytic conception of personality proposes—it would hardly be fair to call them anything but sketches—are historical. More precisely, they are historical laws superimposed on developmental growth patterns. Even some of the laws of perception are historical, in spite of the fact that this is perhaps within psychology the area closest to actual reduction. To put it crudely, it does sometimes make a difference for what we see whether we see it the first or the second time and we have as yet no laws to account for the difference which do not mention the previous experience. The Gestaltists tried to bridge the gap by their trace theory. Its main trouble is that it is not really a theory. To call it one is grievously misleading, since it does not give us the slightest hint, aside from the purely speculative, of what the traces actually are. This so-called theory is therefore merely a promissory note, an affirmation of faith in the eventual discovery of systematic physiological lawfulness in the area. Social science is without doubt systematic in its theoretical aspirations. Negatively, it is one of the main tenets of the theorists in this area that so-called historical explanations, as humanists use the phrase, appeal either to very rudimentary developmental regularities or, more often, merely to empathetic insight. Positively, they feel that if they only knew enough about the present state of a group and had more of the theory they expect to build, they could predict its future without any information about its past. All this goes to show how profoundly our conception of science has been influenced by Newtonian mechanics, that first great triumph of systematic process theory. To repeat, physical and physiological theory are systematic; in social science, theory is at least systematic in intent. Psychology is the exception. Its theory or theories, though surely poor if compared with the older disciplines, are no longer entirely negligible. Yet they are historical; and they are likely to stay so as long as they are genuinely psychological, that is, as long as they are stated in behavioral rather than in physiological terms. What, one may wonder, does this imply for the two reductions in which psychology is potentially involved, once as the reduced theory in its relation to physiology, once as the reducing theory in its relation to social science.

The question is whether systematic laws, namely, the axioms of the makrotheory, can be deduced from historical ones, and conversely. Again, there is no general answer, certainly not in the affirmative. If it can be done at all, it will still depend on the detailed structure of any pair of theories whether it can be done in their case. But can it be done at all? Here we have a mathematical model to fall back on. The mathematical schemata of systematic and historical theories are systems of differential and integrodifferential equations respectively. Schematically, the question is thus under which conditions, if any, one process can be the exact solution of both types of equations. I do not know the answer. I do not even know whether it is known at all. On mathematical grounds of a very general nature, it would seem safe to assume that either process can be approximated by the other. Practically, this is all that is needed. Besides, the mathematical schemata are incomparably more elaborate than any actual theory in these areas is likely to be for some time. Schematically, though, if approximation is the most that can be achieved, it would seem that the odds against reduction between two eventually comprehensive theories, one systematic, one historical, are further increased. On the other hand, the limits of approximation may fit nicely the difference between optimal accuracies of measurement in the two areas. All this is so complicated that I had better stop. Otherwise, I might just be putting words together in ways that sound more or less familiar.

# THE INDIVIDUAL FROM CONCEPTION TO CONCEPTUALIZATION

FRANK A. BEACH

## INTRODUCTION

IN the final decade of the nineteenth century the famous European biologist, Hans Dreisch, described some amazing experiments. Dr. Dreisch was interested in the development of organisms, and his evidence pertained to a definitely unspectacular invertebrate, the common sea urchin. Like all other creatures, the sea urchin begins life as a single fertilized egg. Shortly after it has been fertilized, the sea urchin egg divides to form two cells. A few hours later each of these two cells redivides; and this process continues until a fully formed sea urchin has come into existence.

Herr Dreisch tried out various procedures which he hoped might alter the normal course of development. To begin with, he took sea urchins in the two-cell stage of development and cut them in half, completely separating the cells from one another. He was probably astonished to discover that under optimal conditions each of these two cells divided and redivided until eventually two completely formed embryos were produced.

In later experiments Dreisch found that the usual course of development of the sea urchin, as well as that of many other aquatic organisms is drastically changed when the chemical composition of the surrounding sea water is altered. By adding various inorganic salts to the normal fluid environment Dreisch was able to produce a condition of cyclopia; and in other instances he caused frogs and other amphibians to grow two heads instead of one.

Half a century after Dreisch had reported his discoveries, a psychiatrist, Dr. Rene Spitz, published a report concerning the physical and psychological development of 91 children reared in a foundling home. At the time of Dr. Spitz's original observations all of these children were three years old or younger. During the next two years some youngsters were adopted, but 37 per cent of the original group had died. At the end of this time the 21 survivors who still remained in the home ranged between two and four years of age.

Five of the 21 were totally unable to walk, and only five could walk unassisted. Twelve of the 21 children had not yet learned to eat alone with a spoon, and only one of the 21 was able to dress himself without help. Not a single child was completely toilet trained. Six were totally unable to talk, and only one was capable of using sentences. The marked retardation shown by these children was not limited to their behavior. Although they were all two years or older, only three of them fell within the weight range of the normal two-year-old child. Most of them had the physical appearance of children approximately half of their age.

It is most important to stress the fact that from the time of birth these children had been under excellent medical care. They had received an entirely adequate diet and had not been exposed to any unusual sources of injury or infection. The one atypical aspect of their life histories lay in the almost total lack of social stimulation during the first two years of life. Although they were fed regularly and kept clean, they never had any opportunity to play with adults, or with one another, or with inanimate toys.

In 1947 a young psychologist, Dr. Keith Hayes and his wife Catherine adopted a two-day-old chimpanzee. They named the baby Viki, and brought her up in their home just as they would a human infant. When she was three years old Viki was given a number of psychological tests. In many of these she was the equal of human children her own age, and in all of them she was highly superior to three-year-old chimpanzees that had been brought up in the regular laboratory where these animals were bred.

At first thought the observations of Dreisch, of Spitz, and of the Hayeses may appear to have very little in common. But I have cited them because they all deal with the phenomenon of individual development. Nor does the similarity end here. All three of these studies emphasize the fact that many aspects of development depend upon what happens to the organism during the early stages of life.

Discussions of ontogenesis traditionally separate *prenatal* and *postnatal* differentiation and growth. It is also customary to distinguish between physical and psychological development. One aim of this address is to emphasize the similarities and continuities between prenatal and postnatal, and between physical and psychological development. It is my purpose to elucidate a few simple principles which are pertinent to the interpretation of different kinds of development occurring at different stages in the individual's life span. I shall try to show that wherever and whenever it occurs, development obeys certain common laws. All development, whether it be development of the lens of the eye, or development of abstract concepts of morality, follows an orderly and predictable course and is best understood as the product of mutual interaction between a few classes of basic factors.

## EMBRYOLOGY OF THE VERTEBRATE EYE

Let us begin by considering as our first example the embryological development of that most important sensory structure, the vertebrate eye. At a time when the embryo is little more than a hollow ball, certain groups of cells begin to grow rapidly and to form what eventually will become the spinal cord and brain. Long before the cord has been fully formed it is possible to identify, in that region which will become the brain, two small, laterally placed pits or depressions. When development has proceeded a little further, these pits increase in size, balloon outward, and become the optic vesicles. With continued growth, the optic vesicles are extended closer and closer to the surface of the embryo-to-be, all the time maintaining their connection with the more central brain regions by way of the short optic stalks.

As the optic vesicles approach the surface, they change in structure and take on the shape of a two-layered cup. While the optic cups are forming, a small portion of the outer layer, that is, of the ectoderm, starts to sink inward toward the cup. This migrating group of cells forms into a hollow ball which eventually comes to rest in the cavity of the cup and forms the lens of the eye.

This is a brief and oversimplified account of one small step in normal embryological development, but it typifies the complexity and at the same time the orderliness of differentiation and growth. The sequence of changes leading up to formation of the eyecup and lens seems stereotyped, automatic, almost mechanical. It appears to occur identically in all embryos of the same species and presumably has been occurring in the same fashion for however many millions of years that species may have existed. How are we to account for such a complex yet regular and lawful sequence of phenomena? It was once believed that such an orderly series of events must depend exclusively upon some intrinsic properties of the various cells involved. However, a series of ingenious experiments revealed that this is not at all the case.

### EXPERIMENTS ON THE LENS ORGANIZER

It is now known that the scientist can alter the normal course of development in various ways. For example, by delicate surgical methods one can remove from the surface of the embryo a small patch of tissue, which ordinarily would develop into skin of the belly wall. The next step is to insert this block of ectoderm in a position directly above the developing optic cup, having first removed the cells which normally grow there. When this is done, the tissue that originally was destined to become belly skin will form a lens. And this lens is identical to the one that would have been formed by quite different cells if the experimentalist had not interfered. It is equally feasible to induce formation of a lens in parts of the body where one would not normally grow. This can be done by removing the optic cup and transplanting it to a new location. If a growing optic cup is implanted beneath the surface of the embryo, in a region which normally would develop into skin of the back, a lens will be formed by those cells that lie directly above the transplanted optic cup.

As can be imagined, the operations involved are exceedingly delicate, and one cannot expect success in every experiment. Nevertheless, the results which I have described occur with sufficient regularity to justify the conclusion that the direction which the development of a bit of tissue may take is not determined solely by inherent characteristics of that block of tissue, but depends as well upon its location within the organism. In other words, the structures which groups of cells eventually come to form are in part dependent upon the identity of these cells' neighbors.

It must be stressed that this sort of plasticity is not unlimited. On the contrary, the tissue to be transplanted must be taken at just the right stage in its development, and the transplant must be made at a particular time when the adjacent tissues are capable of exerting their directive influences over neighboring masses of cells.

The concept of *organizer substances* has been invoked to describe the sort of phenomena that I have been talking about. It is believed that the optic cup produces a certain substance, an organizer, which exerts action upon ectodermal tissue causing it to approach the cup and to form a lens. The organizer is produced only for a limited time, and if it is to have any effect upon other cells, the optic cup must be transplanted at the appropriate time. Of equal importance is the fact that the susceptibility of ectodermal tissue to the effects of the optic cup organizer changes through time. Cells that are too far along in their course of development can be transplanted into a region adjacent to the cup, but they will not react to the organizer.

### EXPERIMENTS WITH THE PRIMARY ORGANIZER

Other embryonic organizers are known to exist. For example, the first organizer is probably one produced by a group of cells lying near that region known as the blastopore. From cells in the dorsal lip of the blastopore there comes the organizer which initiates formation of the nerve tube that eventually will become the brain and spinal cord. It has been shown that cells which would normally be involved in formation of some other part of the body can be induced to develop into a spinal cord and brain if they are transplanted into the neighborhood of the blastopore lip.

Furthermore, if tissue is taken from the dorsal lip of the blastopore and implanted elsewhere in the blastocyte, it will induce the formation of a nerve cord and brain in that new location. A classic experiment by Spemann and Mangold involved removing the dorsal lip of the blastopore from one salamander blastocyte and implanting it in a second embryo-to-be. The recipient blastocyte then contained two sources of primary organizer, the transplanted blastopore lip and its own blastopore tissue. The result was the formation of two nervous systems within the same embryo, one induced by the transplant and the other induced by the embryo's own organizer cells. Here again, as in the case of induction of the lens, it is essential that the reacting tissue be transplanted at a specific stage in its own development; and it is also necessary that the transplantation be done while the dorsal lip tissue is actively producing the organizer substance.

The story of experimental control of embryological development is a long and fascinating one, but enough has been said to establish the point I wish to make. This is that the embryological history of any organ or organ system in the body is a joint product of the inherent characteristics of the tissues which formed that organ, and of external influences exerted upon those tissues, either through the activity of other parts of the organism, or in some cases by extraorganismal factors. Throughout the course of morphological differentiation and growth, the basic processes involved are those of interaction between cells or cell masses and their environment. Neither the intrinsic developmental capacities of the cells, nor the activity of external agents is sufficient to explain the final result. Both must always be taken into consideration and both are equally important. One subsidiary principle which is well illustrated by this evidence is that the organism and its various parts change through time with respect to the susceptibility to modification. It seems likely that every cell or group of cells passes through a critical stage of growth during which it is maximally sensitive to outside influences. Having passed this period of high susceptibility the cell or cells are no longer easily changed.

### DEVELOPMENT OF VISUAL PERCEPTION

Let us turn now from the consideration of embryological development and examine the growth of sensory capacities in early life. It is well known that the newborn baby is extremely limited in his visual ability. He cannot fixate his eyes for any appreciable length of time and it seems quite clear that precise pattern vision does not develop for many weeks. Only after several months does the infant become capable of discriminating objects visually and recognizing a familiar face. A part of this gradual improvement is referrable to maturational changes in the visual apparatus. The contribution of *experience* is difficult to assess, because the infant is unable to use speech and his psychological capacities are limited. However, there is evidence proving that visual abilities which might ordinarily be put down to simple maturation are in fact heavily dependent upon early stimulation and experience.

This evidence comes from the study of adult humans after removal of congenital cateracts. Von Senden has summarized a number of cases in which the preoperative condition of the lens was such that detailed vision had never been possible from the time of birth. Following removal of the cateract all of the patients are able to perceive the presence of a figure in the visual field. They can distinguish a figure from its background, but they cannot identify the figure or describe its shape. After a little practice most patients are able to name colors, but from this point on progress is slow and often discouraging.

Some individuals never improve beyond the ability to identify objects in terms of brightness and color.

Others take months or even years to learn to identify simple geometric figures and to read separate letters and numbers. Only in rare cases do these patients master the problem of identifying complex visual patterns such as words, or outline drawings, or human faces. One woman was hospitalized for more than half a year postoperatively, and during this time she was visited daily by two physicians. Six months after the cateract had been removed this patient was totally unable to tell the doctors apart on the basis of their facial characteristics, although she could do so easily in terms of voice or other nonvisual features.

Impressive as it is, this evidence does not constitute conclusive proof of the importance of protracted visual experience for normal visual abilities. In the first place, the absence of the lens can never be completely compensated. Furthermore, these patients characteristically exhibit jerking movements of the eyes, the so-called spontaneous nystagmus. For these reasons it is highly desirable to obtain experimental data on animals in which such factors can be controlled.

This sort of evidence is now available as a result of the careful investigations of Dr. Austin Riesen. Riesen reared young chimpanzees from birth to 16 months of age in a completely darkened room. The two infants in his first experiment were brought into the light at 16 months. At this age they were unable to fixate objects in the visual field, although the normal 16-month-old chimpanzee is visually well developed. Some reflex responses to visual stimulation were present in the dark-reared apes. These reactions included pupillary constriction to bright light, startle response to sudden changes in illumination, and visual following of a large, moving target. Visual responses to complex patterns of light appeared eventually, but not until many hours had been spent in illuminated surroundings.

The chimpanzees did not respond visually to play objects or to their feeding bottles, although both of these were promptly oriented to and grasped if they touched some part of the animal's body. The apes did not blink when a blow toward the face was threatened, and if an object was brought slowly toward the face the first reaction was a startled jump when contact occurred.

Some question was raised concerning structural normality of the retinae and optic discs as a result of total absence of visual stimulation during the early phases of life; and to check this possibility a second experiment was conducted in which three more newborn chimpanzees were put into darkness. One animal was raised for seven months in complete darkness. One was allowed 90 minutes of light every day, during which time the head was covered with a white, plexiglass mask that admitted diffuse light but did not permit pattern vision. The third chimpanzee was given one and one-half hours of patterned light daily.

With minor exceptions, the behavior of the animal reared in complete darkness and the one allowed 90 minutes of unpatterned light per day was similar to that which was shown by the two animals in the original experiment. In contrast, the ape which was allowed 90 minutes of patterned light per day behaved like a normal chimpanzee. He quickly learned to avoid a large yellow-and black-striped disc after receiving one or two mild electric shocks from it; whereas the other chimpanzees were shocked from the same disc twice a day for six and nine days, respectively, before they so much as whimpered when it was shown to them. It took 13 and 15 days before these animals consistently indicated, by some sort of avoidance response, that they saw the disc when it was raised in front of their eyes.

Results of this sort, taken together with the clinical evidence from human beings, strongly suggest that early learning and experience play a very important role in the development of perception by animals and men. It is obvious that the way a person interprets a complex visual stimulus such as a portrait or painting will depend upon his tastes and knowledge gained through experience. It is equally true, but less apparent, that the capacity to see the difference between a circle and a square is also a product of individual learning.

Another investigation conducted at the Yerkes Laboratories of Primate Biology indicates that early learning also plays a role in nonvisual behavior. In this experiment one chimpanzee was reared under conditions in which he could not use his hands or feet to manipulate objects in the environment or to touch himself. The means of prevention was to encase the extremities and distal portions of the limbs in cardboard mailing tubes which permitted limb movement but prevented manual exploration. After the tubes were removed, the animal reared in this fashion did not groom himself in normal chimpanzee style. When a tactile stimulus which he could not see was applied to some part of the body, the experimental chimpanzee failed to bring one hand to the locus of stimulation in the manner which is characteristic of normal apes. Tests which involved prevention of the use of visual cues revealed that the chimpanzee experienced great difficulty in distinguishing between stimulation of his right versus his left hand. His sitting posture was abnormal and in general the behavior was characterized by a number of deficits which seemed directly traceable to the lack of ordinary experience in the use of tactile sensations.

Dr. Arnold Gesell and his co-workers have worked out an extensive schedule describing the development of human infants. This schedule, which has been applied to large numbers of children, clearly reveals that behavioral development involves an orderly and predictable sequence of changes, and that it follows more or less the same course and has the same general tempo in all normal infants. A comparable development schedule for the chimpanzee has recently been described by Riesen and Kinder. There are marked species differences, of course, but the maturational history of the ape resembles that of the human in respect to its general predictability and consistency from individual to individual.

Postnatal progress in muscular control of bodily activity, and increasing precision of sensory-motor coordinations is as regular and consistent as is the prenatal differentiation and growth of the vertebrate embryo. An important factor in this progress is the physical maturation which brings greater strength and increased sensory acuity. But maturation alone cannot account for many of the simplest developmental changes. The evidence I have summarized compels us to conclude that exteroceptive stimulation from the earliest age is of utmost importance; and repeated reaction to such stimulation is an indispensable influence for the development of many normal behavioral capacities. As is the case in embryonic life, normal development rests upon the interaction of intrinsic and extrinsic events and processes.

### EFFECTS OF PRACTICE ON MATERNAL BEHAVIOR

It is customary to classify certain complex patterns of behavior as instinctive. The validity of this taxonomic procedure is open to serious question, but this is not an appropriate occasion on which to debate it. The instinct concept implies that certain kinds of behavior appear normally without the necessity for practice and learning. In the main, the so-called instinctive responses are reasonably constant for a given species, although they vary from species to species. Instincts are ordinarily contrasted with the habits and learned responses which are formed by the individual as a result of personal experience.

One instinctive pattern of behavior which has been studied fairly extensively in the laboratory rat is the maternal care of the young. It has been demonstrated independently by a number of investigators that female rats that have no previous experience in bearing or rearing young are capable of building a nest before the advent of the first litter, cleaning the young as they are born, keeping them together in the nest, nursing and protecting them until they reach the age of weaning.

In an experiment which I conducted a number of years ago female rats were separated from their mothers at the time of weaning and each one was reared in an individual cage where she had no contact with other animals. When they reached adulthood the females were placed with a fertile male just long enough for mating and were then returned to their isolated cages. Approximately five days before the litter was due to be born, each female was placed in a large observation cage and provided with nesting material. The majority of females readily detached paper strips which were hung from the walls of the observation cage and used them to build simple but adequate nests. As soon as the young were born the female took them between her forepaws and licked off the fetal fluids and devoured the fetal membranes. Inasmuch as I knew the entire history of each of the experimental animals I was quite sure that this behavior was properly classified as instinctive, and that it did not depend upon prior experience. More recent studies throw some question upon this naive interpretation.

Dr. Bernard Riess has conducted several experiments in which female rats were reared in an environment containing nothing which could be picked up and transported. He has found that when females of this type become pregnant and give birth to young, they build no nests and are lax in the care of their offspring. Riess's interpretation is that the normal rat has ample opportunity for experience in moving objects about the environment. When this very general sort of experience is denied the animal, there occurs a deficit in the behavior which demands precisely this kind of response. And it is true that in the early experiments, including my own, females were not prevented from doing as much transporting as they wished of food pellets, sawdust, shavings, and other materials commonly available in the living cages.

### EXPERIMENTS ON SEXUAL BEHAVIOR

The sexual behavior of animals is usually classified as instinctive or unlearned. In fact, this type of behavior has often been cited as an excellent example of what is called a "delayed instinct." It is supposed to appear automatically at puberty in response to the physiological changes which occur at this time. It is considered to be the product of physical maturation and to be independent of previous experience. The inadequacy of any such interpretation is revealed by recent experiments on the mating behavior of the laboratory rat.

Two years ago Mr. Jerry Kagan and I conducted an experiment in which male rats were reared in individual cages from the age of weaning. Some of the rats were removed from their isolated cages for a brief period each week and were placed in a large observation cage together with another young rat their own age and size. These weekly, ten-minute periods of contact represented the experimental rats' sole opportunity to form habits of social interaction with another individual of their kind.

When they reached sexual maturity all of the animals were tested with receptive females. The mating performance of the experimental rats was quite abnormal. They were highly excited by the presence of the estrous female and responded to her with the execution of incomplete mating attempts. At the same time, however, there occurred many playful responses of mock fighting, chasing and wrestling which are characteristic of the *immature* rat. The achievement of complete intercourse was so rare that the experimental males practically never reached the sexual climax. The infrequency of coital responses was due to the competing tendency to engage in immature, playful activities. Apparently the extremely artificial conditions of early life favored the development and retention of social habits that were inappropriate in adulthood. Presumably in *normal* development the tendency to react playfully to a second animal appears in late infancy but is gradually replaced by other forms of social interaction. The conditions of our experiment did not provide opportunity for the development of more mature types of behavior and in the absence of this essential opportunity the animals remained fixated at a prepuberal level.

This evidence suggests that abnormal experiences in early life can interfere with the development of normal sexual behavior. It does not prove that the mating of rodents depends upon sexual practice and learning. However, in the higher mammals it is quite probable that biologically-effective intercourse necessitates a considerable amount of sexual experience.

It has recently been reported by Dr. Henry Nissen that young male and female chimpanzees which are sexually mature, but have never had any heterosexual experience, are incapable of engaging in complete sexual relations. Males and females of this species which have been reared under conditions that did not permit heterosexual play do not mate when they are placed together in adulthood. It seems probable that if an indefinite period of exploration and experimentation were permitted, coitus would eventually occur; but under the controlled conditions used by Dr. Nissen, young males and females that were placed together when the female was at the height of her receptive cycle failed to engage in any primary sexual activity in the course of hours of observation. Mating responses are much more likely to occur if at least one member of the pair is sexually experienced.

It appears that normal development of sexual behavior involves both physiological maturation and behavioral experience. Neither factor alone suffices to produce the final product.

### STUDIES OF IMPRINTING

One large body of evidence which emphasizes the importance of experience and early environmental stimulation in the development of social behavior is provided by the studies of the ethologists, or students of animal behavior under natural conditions. One of the first observations dealing with early social conditioning was published by Wallace Craig in 1918. Craig reported that pigeons which had been reared by parents of a different avian species preferred to mate with birds belonging to the same species as their foster parents, and were very slow to form sexual unions with individuals of their own kind.

More recently the German ornithologist, Dr. Konrad Lorenz, has described a process which he calls "imprinting." According to this hypothesis young birds of certain types form strong social attachments very shortly after they leave the egg. Under normal conditions, hatching takes place in the presence of the mother bird, and the young birds soon learn to follow and remain close to the adult female. This tendency is strengthened by virtue of the fact that the mother calls the young to food and protects them against natural enemies.

In a series of naturalistic, semi-experimental studies, Lorenz arranged to substitute himself as a foster parent. He became Father Goose to a brood of graylag goslings. The youngsters were prevented from any contact with adults of their own species and were periodically attended by Lorenz as he crawled about in the grass on his hands and knees and made gooselike noises. The result was that the newly hatched birds accepted their foster parent enthusiastically and followed him about wherever he went. Adult geese held no attractions for the adopted offspring of Dr. Lorenz, and this particular brood of birds retained the strong positive reactions to him for many months.

Other investigators have confirmed in part the initial findings of Lorenz and have shown also that imprinting is not limited to geese but can be observed in several other avian species. In addition, it is possible that a similar process operates in the early social conditioning of those species of mammals which are born at an advanced stage of development and which are by nature gregarious. For example, there are several descriptions of lambs which have been reared by hand and have had no early contact with other animals of their own species. These individuals are said to form social attachments to human beings and to be quite unsheeplike in regard to their behavior in the pasture. They tend to graze alone and do not move with the flock.

Under ordinary conditions the newborn lamb displays a very strong tendency to remain in close contact with its mother. As soon as it can get to its feet, the youngster rubs against the ewe and when she moves away, it follows immediately. At this point any enforced separation elicits immediate distress behavior in the young and the parent. Somewhat later in development the lamb may leave its mother's side briefly to investigate its environment or to play with another young sheep. Eventually the positive social response generalizes from the mother to include all other members of the flock. This orderly course of development in social behavior is dependent jointly upon inherent aspects of the sheep's behavioral capacities, and upon individual experience. The final result is a product of interaction between these two sources of influence. Furthermore, as is true in experimental embryology, susceptibility to the influence of external agents varies through time. Lambs deprived of the usual contact with other sheep in the early stages of life do not become normal sheep even though opportunity for such contact is provided at a later developmental stage.

## EARLY EXPERIENCE AND COGNITIVE DEVELOPMENT IN CHILDREN

Thus far I have been speaking about the effects of early experience upon the development of perceptual functions and upon the ontogenesis of emotional, instinctual, and social behavior. Another axis of development which is of great importance is, of course, the development of intelligence or of cognitive capacities. When intelligence tests were first introduced, and the IQ was invented, there was a great deal of speculation and argument concerning the so-called *constancy* of the intelligence quotient. In the beginning, some proponents of the testing movement insisted that the intelligence test measured innate intellectual capacity and was independent of the effects of learning and experience. It has since become apparent that this extreme view is untenable, and it is questionable whether any psychological test is completely immune to the effects of certain widespread, not to say universal, forms of early life experience.

The Swiss psychologist, Piaget, has suggested that the way in which children think about the surrounding world and about themselves undergoes a regular development as the child grows older. The individual's way of thinking passes through several distinctive stages before adult ideas are attained. The theory is not that the child grasps adult ideas with different degrees of adequacy, but rather that it develops ideas which differ *qualitatively* from those of the adult.

The primary stage of thought development differs from the intermediate and final stages in that it is relatively unaffected by the culture into which the child is born. The child's primary concepts, be they concepts of names or of windstorms or any other phenomena are generated by the child himself. Primary concepts reflect the child's perception of, and reflection upon, its environment; but they are relatively unaffected by adult ideas because at this age the child is not in good contact with adult ideas, and in any event is unable to understand them.

Piaget believes that these primary ideas are arrived at independently by each succeeding generation, and eventually discarded in favor of the adult ideas favored by the particular culture in which the individual grows up. Furthermore, many of the primary ideas are the same in different cultures and in different generations. This is so because of certain assumed constants in the human psychological processes, and because of certain environmental universals, that is, certain factors and phenomena universally present in the worlds of all children.

These hypothetical environmental universals may be likened to the many constants present in the environment of the human fetus. A particular concentration of oxygen, a given amount of calcium, an essential supply of iodine—these and many other things must be available to the embryo if it is to develop normally. However, since they are almost always present, their contribution to normal growth and differentiation is easily overlooked. Only when one or another of the essential items is lacking is its essentiality apt to be recognized.

Carrying the analogy back to psychological development, we can ask the following question: If Piaget's thesis is sound, what are the cultural universals that contribute to the beginning of thought and the derivation of primary concepts? Many answers suggest themselves and one of the most obvious is the stimulation afforded all children of every society by contact with other human beings. There is some reason to believe that this is an indispensable factor in normal psychological and perhaps even physical growth.

At the beginning of this address I cited certain findings reported by Dr. Rene Spitz and they bear directly upon the point under examination. Dr. Spitz studied two populations of European children. One group lived in a home for foundlings. The other was established in a nursery attached to a women's prison. Tests administered very early in life indicated that the two groups did not differ significantly in intellectual endowment and were comparable as far as physical characteristics were concerned. During the first phase of life the development of children in both groups was similar, but at approximately the fourth month the developmental curve for foundling home children decelerated, while that of the children in the nursery home continued to rise normally. In addition, although both institutions maintained adequate health precautions, the physical development of the foundling home children was markedly retarded.

So far as Dr. Spitz could ascertain, the crucial difference between the groups lay in the amount of social stimulation the children received. Up until the fourth month babies in both homes were breast fed and after that time the foundling-home group was fed by bottle. Infants in the nursery were fed by their own mothers, who were prisoners, and who were allowed several hours each day to spend with the children. During this time most of the children in the nursery received a great deal of affectionate care and attention. Children in the foundling home were attended by a staff of nurses, too few in number to permit any extra attention outside of seeing to it that the children were not allowed to go hungry and were kept clean. In the foundling home each child remained in its crib all of the time. The sides and ends of the crib were draped with sheets so that all a child could see for most of the day was a portion of the ceiling directly above him. He could not see other children, and only saw an adult while he was being fed or cleaned. Nursery children were given playthings and toys, but these were lacking in the foundling home.

I have already summarized Dr. Spitz's observations when he returned to the foundling home when the children who were still there were between two and four years of age. The deterioration which he had observed in the first year of life had progressed even further. He found in addition, that improving the environmental conditions after approximately the fifteenth month of life had no beneficial effects. For example, some children were put into a different ward with older individuals. There, they lived in a large, sunny room without partitions. Several nurses were present at all times and the children were taken from their cots and allowed to play about on the floor. Thus they had much more social stimulation than they previously experienced, but the defects due to earlier isolation proved irreparable. Dr. Spitz sums up his own findings as follows:

> It would seem that the developmental imbalance caused by the unfavorable environmental conditions during the children's first year produces a psychosomatic damage that cannot be repaired by normal measures. Whether it can be

repaired by therapeutic measures remains to be investigated. We have advisedly spoken of psychosomatic damage. From the figures given above it can be seen that quite apart from the inadequate psychic and physical development, all these children showed a seriously decreased resistance to disease, and an appalling mortality. Those who survived were all far below the age-adequate weight reached by normal children of comparable age.

In contrast, the children living in the nursery, although the oldest of them was half a year younger than the youngest child in the foundling home, and two-and-one-half years younger than the oldest in the foundling home, all ran lustily around the room; some of them dressed and undressed themselves; they fed themselves with a spoon; nearly all spoke a few words; they understood commands and obeyed them; and the older ones showed a certain consciousness of toilet requirements. All played lively social games with each other and with adult observers. Their physical condition was good and during the time covered by the study not a single child from the nursery died.

In interpreting these findings it is necessary to recognize that the social isolation of children in the foundling home was so pronounced that it can scarcely be matched by any other carefully documented account on child rearing. It is true that in some societies the mother pays but little attention to her infant, and individual cases of serious neglect are occasionally encountered in our own culture. But almost never is an infant left to spend practically all of its time without any human contact. If a society compels the mother to work in the fields or allows her to be away from her child for most of the time, alternate provisions for its care are made. In some cultures it is traditional for older siblings to care for the youngest members of the family, and in our society either a relative or a hired attendant is with the young child most of the time.

It seems well within the bounds of possibility that one of the universal conditions affecting human development is the impact of a considerable amount of stimulation from the environment, particularly the opportunity for social interchange with other human beings. Like the iodine content of the prenatal environment, this postnatal environmental condition is so widespread and universal that its importance is easily overlooked. If this line of reasoning is valid, the similarity between pre- and postnatal, and physical and psychological development can safely be described as fundamental and important. The gradual increase in intellectual powers is attributable, not to environmental support and direction, nor yet to inherent maturational processes, but rather to the continual interaction of both types of factors.

## COGNITIVE DEVELOPMENT IN ANIMALS

If experience makes important contributions to normal intellectual development, and if experiential deficits can prevent or retard such development, it is at least conceivable that by increasing the appropriate kinds of stimulation or experience one might induce a supernormal degree of intellectual ability. This in essence is what we try to achieve with our formal educative machinery and the informal tutelage of our own children. But the effects of an "enriched" early environment are most clearly revealed in experimental studies and observations on various kinds of animals.

A number of years ago Dr. Henry Nissen studied the behavior of chimpanzees in their native habitat. One of the most interesting conclusions reached as a result of this work was that the free living chimpanzee is less well developed intellectually than is his relative who has been born and reared in the experimental laboratory. The environment in which these animals normally exist is a beneficent one. It poses relatively few problems for the individual to solve and makes minimal demands upon his learning ability. The climate is gentle and even, food is abundant, and natural enemies are scarce. Accordingly, the average chimpanzee can get along quite well without ever extending himself intellectually.

In contrast, the ape that is bred and reared in the psychological laboratory is faced with many man-made problems that never trouble his wild cousin. His food is provided, but he often has to work for his dinner, and in solving the various problems put before him by the experimentalist the animal gradually acquires a degree of sophistication which far exceeds that of the free living ape.

Comparisons of this sort suggest the interesting hypothesis that the normal or average psychological status of a given species is in part dependent upon the environmental demands placed upon all members of that species by nature. If this were true, then the possibility mentioned earlier could be investigated. It might be feasible to elevate individuals far above the norm of their species by artificially increasing the problematical aspects of the life circumstances and multiplying the situations in which learning is possible and necessary.

The conditions of human life are rendered highly complex by the existence of cultural or social problems that do not confront members of other species and it is not unlikely that man's unique intellectual characteristics depend in appreciable measure upon this fact. One way of assessing the effects of this factor would be to expose the young of some other species to the same environmental opportunities and necessities which confront the growing human child. This has been attempted by several investigators; and one of the most recent and ambitious of such projects is one which I mentioned at the beginning of this address. I refer to the work of Keith and Catherine Hayes at Orange Park, Florida. Their chimpanzee, named Viki, was adopted a few days after birth and has now spent nearly six years in their home. Insofar as it was possible and consonant with the major purpose of their experiment, Dr. and Mrs. Hayes have reared Viki as they would a human child. The over-all plan necessitated many tests and observations to which children are not subjected. When Viki appeared to be markedly inferior in some respect, special training was usually given, to determine whether the deficiency could be overcome. During much of her life such training took place at mealtime so that she worked for most of her food.

An extensive diary has been kept of Viki's activities, and she has been compared with children and with laboratory chimpanzees in a variety of formal tests. The findings published to date do not go beyond Viki's status at three years of age. In general, she has closely paralleled the development of a normal human child, and her interest and abilities appeared in roughly the same sequence and at about the same rate. Her social behavior is that of an extrovert; she seeks the company and attention of people and is very friendly although somewhat aggressive with those who allow her to dominate them. Like human children Viki spends most of her time in play. However, her play is much more athletic and her skill far surpasses that of a child of the same age.

In *contrast* to human children, Viki is much less vocal. Unlike the three-year-old boy or girl she does not babble or chatter. She does not appear to be seriously retarded in interest or capacity to deal with concrete objects. She scribbles, cuts with shears, builds with blocks and Tinker Toys, hurls and catches balls, operates light switches and door handles, all with about the same skill and enthusiasm of most three-year-old humans. One of her favorite toys at three years of age is the telephone which she holds to her ear while dialing a few numbers at random.

Viki seems to have about as much preference for social play as do children. She leads human beings about by the hand, coaxes them to tickle her, begs for piggy-back rides, brings toys which are used in social games, and enlists human aid in exploiting new gadgets by firmly placing the human's hands at the appropriate spot. She is adept at social imitation and, just like a human child, she copies her adopted parents' routine chores. She dusts, washes dishes, sharpens pencils, saws wood, hammers and sandpapers furniture, paints woodwork, and presses photographs in books.

In an experimental analysis of her imitative behavior Viki was required to solve six problems which were also given to four human children of about the same age. Four of these six problems were presented to a laboratory raised chimpanzee nine months older than Viki. These problems in imitation involved obtaining a toy or piece of candy in various ways, such as throwing a ball at the toy to knock it down, pushing it out of a long tunnel with a rod, or by operating a light switch on the wall which released the prize magnetically from the ceiling. In other problems the incentive was placed in a box which could only be opened by using a stick to strike a distant string, by burning a nearby string with a candle flame, or by pulling three levers in a certain sequence.

The chimpanzees and children were allowed to attempt a solution first, to be sure that it could not be done readily by insight or by trial and error. Then the experimenter demonstrated the proper procedure, and allowed the subject to try again; if she was unsuccessful, additional demonstrations were given. There was little difference between the performance of Viki and the children—they usually solved these problems within a few trials. The laboratory chimpanzee solved only one, and his failure with the others seemed due, at least in part, to his lack of skill in manipulating the apparatus.

When Viki is given psychological tests that do not involve language, they indicate that her general intelligence has developed at about the same rate as that of a human child. At three years she performed with appropriate skill on such test items as form boards, peg boards, picture puzzles, block piling, and buttoning. However, she fails completely on tests of verbal intelligence. In fact, language is the one field of behavior in which Dr. and Mrs. Hayes have so far been able to find a large, clear-cut, and important superiority of man over chimpanzee. In summarizing their first three years of observation, the Hayeses came to the following conclusion:

> If we assume . . . that Viki's mental development will continue to parallel that of man to maturity—as it appears to have done for the first three years—then our results strongly suggest that the two species are much more alike, psychologically, than has heretofore been supposed. They suggest, in fact, that man's superior ability to use language may be his only important *genetic* advantage. This one genetic advantage makes further advantages possible, however, since language is a means of sharing knowledge. Intelligence depends upon three factors: the individual's innate capacity, his personal experience, and the experience he acquires second hand, through communication with others. This last factor, while unimportant to most species, makes man less dependent upon individual abilities, by providing him with a more effective "group intelligence."

In spite of the fact that Viki could not master and use language, she did develop learning and problem-solving ability to a level considerably above that of the ordinary chimpanzee. These superior attainments were due in part to the continuous stimulation and tuition provided in the home environment. Combining this evidence with the observations of Dr. Spitz leads to the conclusion that intellectual development depends upon certain nonexperiential variables, such as those that differentiate man and chimpanzee, and upon certain forms of experience occurring early in life. Serious deficiences are apt to appear if experience is impoverished; and enrichment of early experience is essential if development is to result in full achievement of the individual's potentialities.

### SUMMARY AND CONCLUSIONS

I hope that I have successfully laid the groundwork for four important generalizations about development. The first generalization states that development always depends upon certain, crucial external forces which act upon the developing system. The second is that development is equally dependent upon inherent characteristics of the system, including its sensitivity to environmental influences. The third is that this sensitivity or capacity for response is not constant or stable, but tends first to rise to a maximum and then to decrease and disappear so that the critical extrinsic factors can exert their normal effects only if they are present when responsivity of the reacting system is high. Later in development the same agents may be without effect. The fourth and final generalization is that all of the foregoing ones apply to all aspects of organic development, from the differentiation and growth of the embryonic neural tube to the gradual formation of basic emotional patterns and characteristic habits of thinking.

The range of phenomena, the multiplicity and variety of systems which obey the same, fundamental, ontogenetic laws is at first thought astonishing. How has it come about that development of the eye in the human embryo, and the development of primary ideas in the young child have so many features in common? The answer is that morphological and psychological development belong on the same continuum, and both represent end products of the same evolutionary process. Both have been exposed to and shaped by the selective forces or organic evolution.

These studies of development re-emphasize an oft-stated but important fact mentioned at the beginning of this address and worthy of repetition before an audience which includes specialists in several areas of biological and social science. There is no fundamental discontinuity between pre- and postnatal existence, nor between the physical and psychologic aspects of the individual.

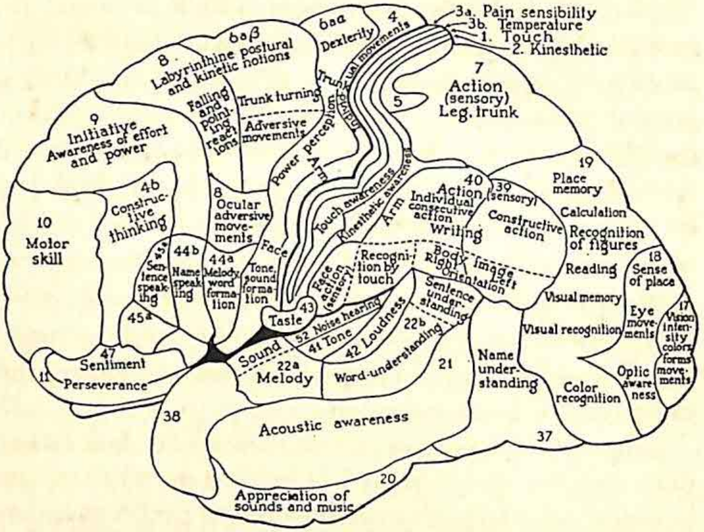
# TOWARD A SCIENCE OF NEUROPSYCHOLOGY

## (*Method and Data*)

KARL H. PRIBRAM

DR. PATTON asked me to discuss with you the relationship between neurophysiology and psychology. With the increasing popularity of the "interdisciplinary approach" there would be no apologia necessary for a science of neuropsychology were it not for the bad repute into which this area of investigation has fallen. Such well-deserved infamy stems, in part, from the dualism which has plagued all of the behavioral sciences during the past 50 years and, in part, from the *excessive* "psychologizing" of physiologists and "physiologizing" of psychologists which fills our journals and monographs. The first figure serves to illustrate the results of such schizoid endeavors.

The deficiencies of the conceptualizations diagramed here become obvious once they have been pointed out. What psychophysicist would assign the same numeral to different classes or assign different numerals to the same class? Yet, flagrant disregard of this simple rule of the most elementary of scaling techniques pervades practically every cytoarchitectonic study and is shown at its worst in Figure 1. What biologist would, in his own field, classify together such diverse categories as ocular adversive movements, optic awareness, vision intensity, color recognition, place memory, constructive thinking, and constructive action, without some



LOCALIZATION OF THE FUNCTIONS OF THE CEREBRAL CORTEX ON ANATOMIC LINES. OUTER SURFACE
From Kleist, K: Kriegsverletzungen des Gehirns, p. 1365.

FIGURE 1. "Localization of function" in the human brain according to a recent authority. See text for "what's wrong" with this figure.

referent of internal consistency and some attempt at ordinal ranking? Finally, where is there available a discussion of the reliability and the validity of the techniques used to construct this monstrosity? The vast differences between various textbook diagrams and the differences between these and our clinical experience suggest the answer to this question.

But what of the experimental studies which have dealt with the relation of brain and behavior? Many such studies using behavioral measures have manipulated environmental conditions and inferred brain function. Other studies have manipulated the central nervous system and measured electrical, histological, or physiological (e.g., movement, blood pressure) responses and inferred a relationship to behavior. Such inferences appear to suffer from the paucity of data accumulated thus far. Some studies have manipulated the brain and measured behavior; these often suffer from the limited applicability of the specific findings. In an attempt to overcome these difficulties, the type of study reported here was undertaken: In these experiments *both* the central nervous system *and* environmental conditions were manipulated and the interaction of these manipulations with the behavior of the organism has been measured.

Since this approach is still in its infancy, data rather than laws will be presented. The data describe the relationships between the manipulations performed (independent variables) and behavior (the dependent variable); it seems premature to attempt systematization of the interrelationships of these independent and dependent variables and thus to formulate laws or concepts. When such laws are formulated, they will, of necessity, be within the framework of a behavioristic psychology. The problem of relating such scientific laws to "private experience" (or Gestaltists' "phenomena") is a problem which behavioristic psychology shares with other sciences and lies beyond the scope of this conference.

Since this approach considers the biology of the organism as one of several classes of independent variables determining behavior, a necessary first step toward a science of neuropsychology (by definition, a reductive science) is a description of the central nervous system in terms other than those defining relationships to the dependent variable (behavior) used in the neuropsychological experiments. Such description is the task of neuroanatomy and classical neurophysiology. For this occasion, I have chosen a description based on thalamocortical anatomy, though one based on cytoarchitecture, "evoked potential" studies, strychnine "neuronography," or a combination of these might have served as well.

Thalamocortical systems may be classified according to whether the thalamic component receives its major afferents from within or from outside the thalamus. The term "intrinsic" has been applied by Rose and Woolsey[17] to those thalamic nuclei which do not receive their major afferents from outside the thalamus. Thalamocortical systems receiving extrathalamic afferents are of two types: those receiving spinal and mesencephalic afferents, and those receiving diencephalic fibers. The former (often called the "primary projection systems") are hereinafter called "extrinsic," following Rose and Woolsey; the latter are most usefully considered under the heading "rhinencephalic."[11] Two examples of current investigation of the intrinsic systems and one example of those of the rhinencephalic systems will be presented.

Figure 2 presents the surgical manipulations of the neural variable in these experiments. Represented are the reconstructions of the cerebral hemispheres of 40 monkeys. The lesions were made, in most instances, on the basis of criteria other than those defining the thalamocortical relationship, a consideration which need not enter this presentation. All diagrams are made by transferring to standard brain outlines the actual

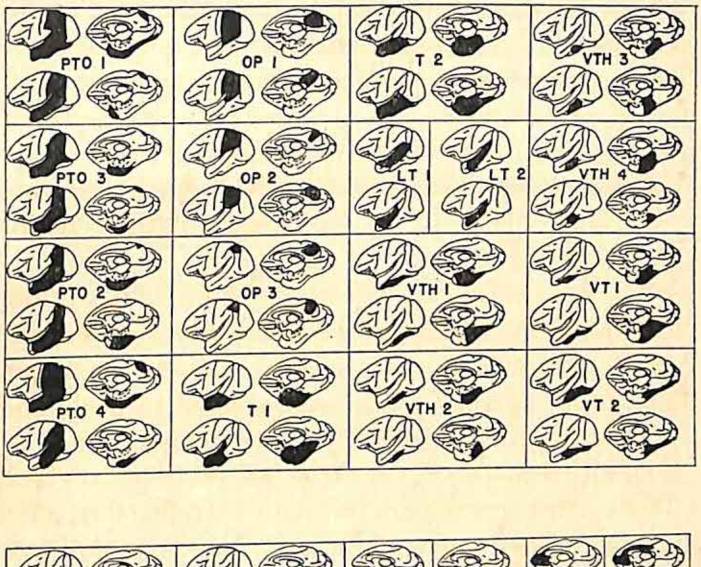


FIGURE 2. Schematic representation of locus and extent of resections performed in 40 monkeys used to relate specific neural systems to specific behavioral processes. (The original reconstructions of the brains of these animals appear in References 5, 6, 8, 9, 10, 14.)

reconstructions from serial sections of the lesioned hemispheres.

In order to decide upon a relevant dependent vari-

SIMULTANEOUS VISUAL CHOICE REACTION

OPERATES WITHOUT DEFICIT

| | Pre | Post |
|---|---|---|
| OP 1 | 200 | 0 |
| OP 2 | 220 | 0 |
| OP 3 | 380 | 0 |
| LT 1 | 390 | 190 |
| LT 2 | 300 | 150 |
| H 1 | 210 | 220 |
| HA | 350 | 240 |
| FT 1 | 580 | 50 |
| FT 3 | 50 | 0 |
| FT 4 | 205 | 0 |
| FT 5 | 300 | 200 |
| FT 6 | 250 | 100 |
| DL 1 | 160 | 140 |
| DL 2 | 540 | 150 |
| DL 3 | 300 | 240 |
| DL 4 | 120 | 100 |
| MV 1 | 110 | 0 |
| MV 2 | 150 | 10 |
| MV 3 | 290 | 130 |
| MV 4 | 230 | 10 |
| MV 5 | 280 | 120 |
| CIN 1 | 120 | 80 |
| CIN 2 | 400 | 60 |
| CIN 3 | 115 | 74 |
| CIN 4 | 240 | 140 |

OPERATES WITH DEFICIT

| | Pre | Post |
|---|---|---|
| PTO 1 | 120 | 272 |
| PTO 2 | 325 | F |
| PTO 3 | 180 | F |
| PTO 4 | 120 | 450 |
| T 1 | 940 | F |
| T 2 | 330 | F |
| VTH 1 | 320 | F |
| VTH 2 | 370 | F |
| VTH 3 | 280 | F |
| VTH 4 | 440 | F |
| VT 1 | 240 | F |
| VT 2 | 200 | F |
| VT 3 | 200 | 890 |
| VT 4 | 410 | F |
| VT 5 | 210 | F |

NON-OPERATE CONTROLS

| | | |
|---|---|---|
| C 1 | 790 | 80 |
| C 2 | 230 | 20 |
| C 3 | 750 | 20 |
| C 4 | 440 | 0 |

FIGURE 3. Pre- and postoperative scores on a simultaneous visual choice reaction of the animals whose brains are diagramed in Figure 2 indicating the number of trials taken to reach a criterion of 90 per cent on 100 consecutive trials. Deficit is defined as a larger number of trials taken in the "retention" test than in original learning. (The misplacement of the score H 1 does not change the over-all results as given in the text and in the following figures.)

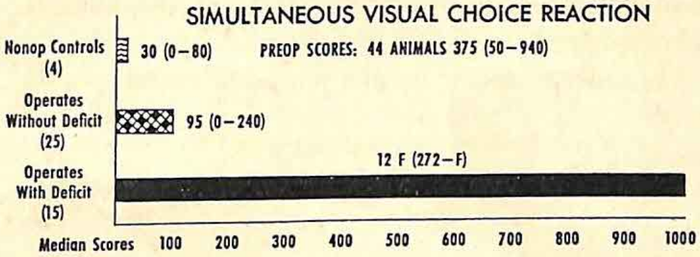


FIGURE 4. Bar graph of median scores of the groups delineated in Figure 3. The number of animals per group is indicated below group name; the range from which median scores are taken appears in parentheses next to the median.

able, approximately 30 different behaviors were observed and quantified. Those behaviors which were affected by some lesions and not by others were then chosen for further investigation. Our first example of such behavior is the visual choice reaction or visual discrimination task.

Figure 3 gives the individual animal's pre- and postoperative scores in a visual choice reaction in which painted patterns were used as cues. Figure 4 summarizes these results. Scores were classified into deficit and no-deficit on the basis of whether an animal took longer to relearn the task postoperatively than to learn it preoperatively. As can be seen, there is no overlap in scores between the group with no-deficit and that with deficit; in fact, the latter group contains 12 of 15 animals which *never* relearned the task even though 1,000 trials were given postoperatively (preoperative mean for learning was approximately 375).

Figure 5 groups the lesions of the animals with deficit and those without deficit. *A* shows the summed area of

## VISUAL CHOICE REACTION

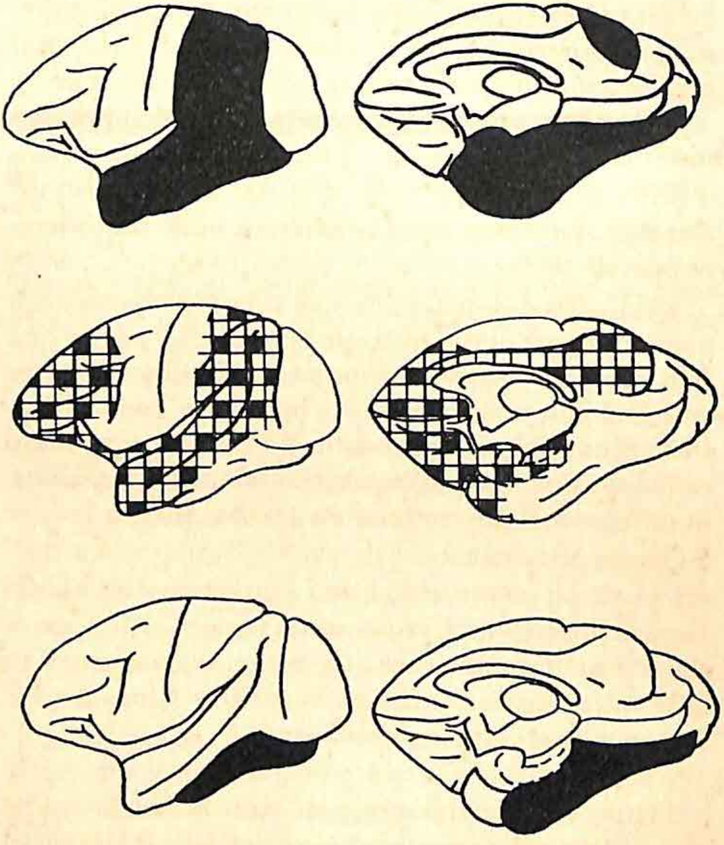


FIGURE 5. The upper diagram *A* represents the sum of the areas of resection of all of the animals grouped as showing a deficit in Figure 3 and Figure 4. The middle diagram *B* represents the sum of the areas of resection of all of the animals grouped as showing no-deficit in Figure 3 and Figure 4. The lower diagram *C* represents the intersect of the area shown in black in the upper diagram and that *not* checkerboarded in the middle diagram. This intersect represents the area invariably implicated in visual choice behavior in these experiments.

all of the lesions which produced deficit; *B* the sum of the area of all of the lesions which failed to produce a deficit in visual discrimination performance. *C* represents the intersect of the area shown in *A* with the total area *not* shown (checkerboarded) in *B*. This may be considered an approximate minimal locus implicated in visual choice behavior in the 40 lesioned animals. This locus approximates that of one of the posterior intrinsic systems and will be referred to as the "inferotemporal" sector.

Having established a selective relationship between a lesion in one of the intrinsic systems and a restricted portion of the behavioral spectrum, we proceed to investigate the environmental conditions upon which this relationship is dependent. For instance, we have called the task a visual choice reaction. Is performance of other visual discriminations affected by this lesion? So far, experiments have shown performance of a variety of visual object, color, and brightness discriminations to be altered.[5, 6] What would happen if in place of the visual discriminanda, their logical analogues in somesthesis were substituted? Would the same, or a different, cortical area be implicated?

Figure 6 shows the results of an experiment where the visual choice reaction was compared with a task in which vision was excluded and a solution of the problem depended on *handling* the cues. Two intrinsic systems were surgically invaded—the inferotemporal and the occipitoparietal. As can be seen, lesions of the occipitoparietal sector fail to interfere with visual choices but affect those based on somethesis, whereas the lesions involving the inferotemporal sector interfered selectively with the visual.[8]

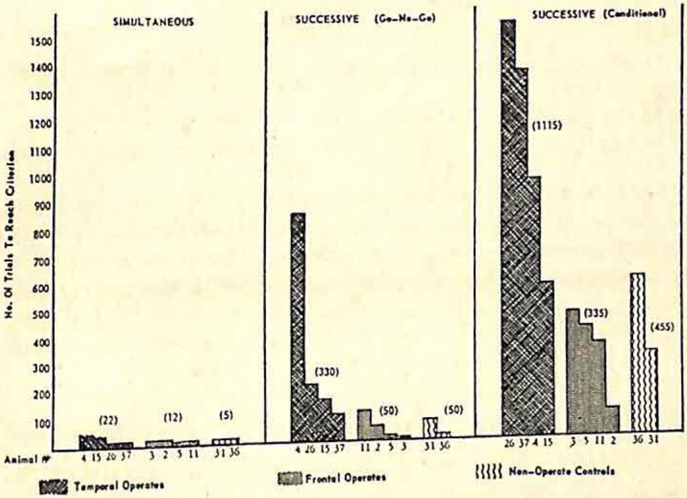
FURTHER BEHAVIORAL ANALYSIS
OF THE PTO CORTEX

| | P48 | P46 | P49 | T44 | T45 |
|---|---|---|---|---|---|
| Visual<br>10 (0-70) | 0 | 0 | 0 | (500) | (500) |
| Somatosensory<br>60 (0-100) | 460 | 120 | 350 | 70 | 50 |
| New Somatosensory | (1000) | (1000) | (1000) | 320 | 260 |

FIGURE 6. Comparison of retention scores of inferotemporal *T* and occipitoparietal *P* operates on a visual and somesthetic task in which logically analogous cues (+ vs. 0) were used. The mean and range of the preoperative retention scores appear under the title of the task. The scores on the "new somatosensory" task indicate original postoperative learning of a length discrimination. Parentheses indicate *failure* to reach criterion in the number of trials given.[8]

If it can be stated that the decrement in performance is restricted to the visual choice reaction, and other experiments on taste,[1] conditioned avoidance,[15] and delayed response[2, 6, 9, 10, 14] support this contention, we are faced with a second cerebral "visual" system. Thus, in addition to the extrinsic (geniculo-striate) system, there is at least one intrinsic system which functions selectively within this modality. It becomes important, therefore, to distinguish between the functions of the extrinsic and intrinsic visual systems. For example, resections within the former, that is, of the striate cortex, lead to field defects; those of the latter, the inferotemporal cortex, do not. Other studies which specify such differences have been completed or are in progress and will be reported elsewhere.[3, 19]

Today, I wish to limit myself to one other aspect of the relationship of inferotemporal lesions to visual choice behavior.[13] Figure 7 describes an experiment in



VISUAL CHOICE REACTION: SIMULTANEOUS VS. SUCCESSIVE

FIGURE 7. Comparison of learning scores of three groups of animals (inferotemporal operates, anterofrontal operates, and non-operate controls) in a simultaneous and two types of successive tasks in which the *same* cues were used. The increment of impairment of the inferotemporal group, as compared with controls, appears roughly proportional to the increasing difficulty of the task for controls.[13]

which animals were taught to choose one of two discriminanda (an ashtray and a tobacco tin) presented simultaneously. The animals were then tested in situations in which these identical cues were presented successively, and the performance of inferotemporal operates was compared with that of control operate and non-operate control groups. Here, as in the experiments of Riopelle and Ades,[16] and of Mishkin,[5] inferotemporal operates have progressively greater difficulty in a series of tasks graded in "distinctiveness" as measured by the difficulty of the task for the control groups. In this instance, however, "distinctiveness" is not dependent on the physical dimensions of the cue, but

CLASSICAL DELAYED REACTION

| Operates Without Deficit | Pre | Post |
|---|---|---|
| PTO 1 | 680 | 270 |
| PTO 4 | 1020 | 540 |
| T 2 | 670 | 120 |
| VTH 1 | 100 | 0 |
| VTH 2 | 60 | 0 |
| VTH 3 | 560 | 0 |
| VT 1 | 290 | 20 |
| VT 2 | 130 | 0 |
| VT 3 | 740 | 330 |
| LT 1 | 140 | 0 |
| LT 2 | 140 | 0 |
| H 1 | 350 | 50 |
| HA | 170 | 140 |
| FT 3 | 200 | 100 |
| FT 4 | 300 | 0 |
| FT 5 | 750 | 300 |
| FT 6 | 1250 | 400 |
| CIN 1 | 820 | 360 |
| CIN 2 | 600 | 150 |
| CIN 3 | 215 | 150 |
| CIN 4 | 345 | 180 |
| MV 1 | 630 | 60 |
| MV 4 | 590 | 240 |
| MV 5 | 380 | 230 |

| Operates With Deficit | Pre | Post |
|---|---|---|
| DL 1 | 290 | F |
| DL 2 | 210 | F |
| DL 3 | 590 | F |
| DL 4 | 240 | F |
| MV 2 | 610 | 960 |
| MV 3 | 430 | 750 |
| PTO 3 | 530 | 630 |
| T 1 | 60 | 90 |

| Non-Operate Controls | | |
|---|---|---|
| C 1 | 10 | 110 |
| C 2 | 590 | 0 |
| C 3 | 230 | 250 |
| C 4 | 440 | 330 |

FIGURE 8. Pre- and postoperative scores on delayed reaction of animals whose brains are diagramed in Figure 2, indicating the number of trials taken to reach a criterion of 90 per cent on 100 consecutive trials. Deficit is defined as a larger number of trials taken in the "retention" test than in original learning.

rather on the situation in which these cues are imbedded. Thus, no selective relationship between the visual discrimination impairment and either of these two classes of environmental variables (cue dimension, situation) is established. I believe this lack of a simple relationship between the physical dimensions of cues and the performance of monkeys with inferotemporal lesions will differentiate these results from those obtained when the extrinsic (geniculo-striate) visual system is invaded. Thus, the distinction between such concepts as "agnosia" (which might account for the results of the "situational" experiment) and "acuity loss" (which might account for the results found on varying the physical dimensions of the discriminanda), which have been traditionally employed to explain the disparate effects of lesions in the extrinsic and intrinsic systems, may be revised in more precise terms allowing interdisciplinary translation.

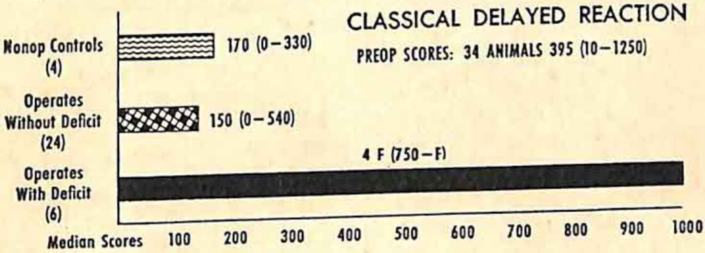


FIGURE 9. Bar graph of median scores of the groups delineated in Figure 8. The number of animals per group is indicated below the group name; the range from which median scores are taken appears in parentheses next to the median.

A *second* example of this approach to the functions of the intrinsic systems is presented in Figure 8 which

DELAYED REACTION

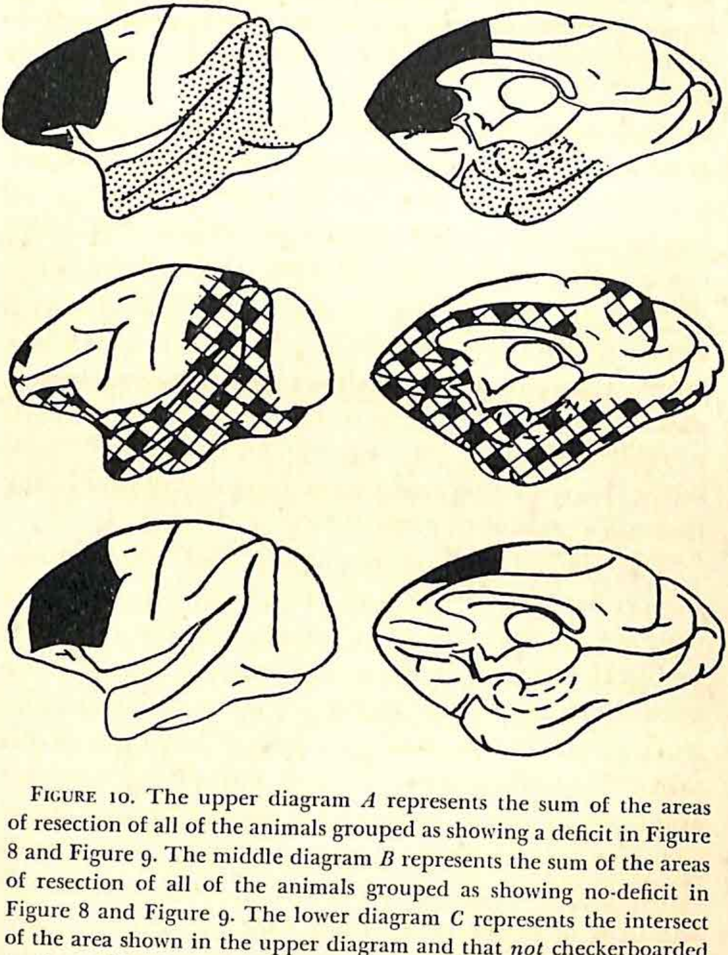


FIGURE 10. The upper diagram *A* represents the sum of the areas of resection of all of the animals grouped as showing a deficit in Figure 8 and Figure 9. The middle diagram *B* represents the sum of the areas of resection of all of the animals grouped as showing no-deficit in Figure 8 and Figure 9. The lower diagram *C* represents the intersect of the area shown in the upper diagram and that *not* checkerboarded in the middle diagram. This intersect represents the area invariably implicated in delayed reaction performance in these experiments. (Note that resections within the area stippled in the upper diagram occasionally result in "deficit" as defined here. However, note also, that a similar "deficit" appears in the non-operate controls in Figure 8.

This finding resolves the discrepancies regarding previously described occasional occurrence of deficit on delayed reaction following posterior cortical resections.[2,4] For the purposes of a "localization" procedure, the delayed alternation task appears to be more reliably retained. Nevertheless, as demonstrated here, the results of delayed reaction experiments may still be useful.)

shows the scores in the delayed reaction made by the animals with the lesions presented in Figure 2. Figure 9 summarizes these data on the basis of animals with and without deficits defined in the same way as in the case of visual choice reaction. Figure 10 shows in *A* the sum of the area of the lesions of the animals with deficit and in *B* the sum of the area of the lesions of the animals without deficit; *C* shows the intersect of area *A* and the area *not* included in *B*. This area corresponds roughly to another intrinsic system, the anterofrontal sector. We are, thus, ready to investigate another of the intrinsic cerebral systems.

The delayed reaction may be manipulated in a manner similar to that which we used in the visual choice reaction. Figures 11 and 12 present the results of such manipulations.[7] *A* shows the difference in performance between animals with anterofrontal resections and control operates in the traditional delayed reaction. In this task the animal chooses the cup containing a peanut from one of two identical cups, on the basis of a cue presented sometime prior to opportunity for response. This cue is not present during the delay period or at the time of response. *B* shows that (1) when the predelay cue is varied from showing a peanut (or object) to the right or to the left of the animal to showing a peanut or a bare hand (or two distinct objects), and (2) when the conditions of response are varied to oppor-

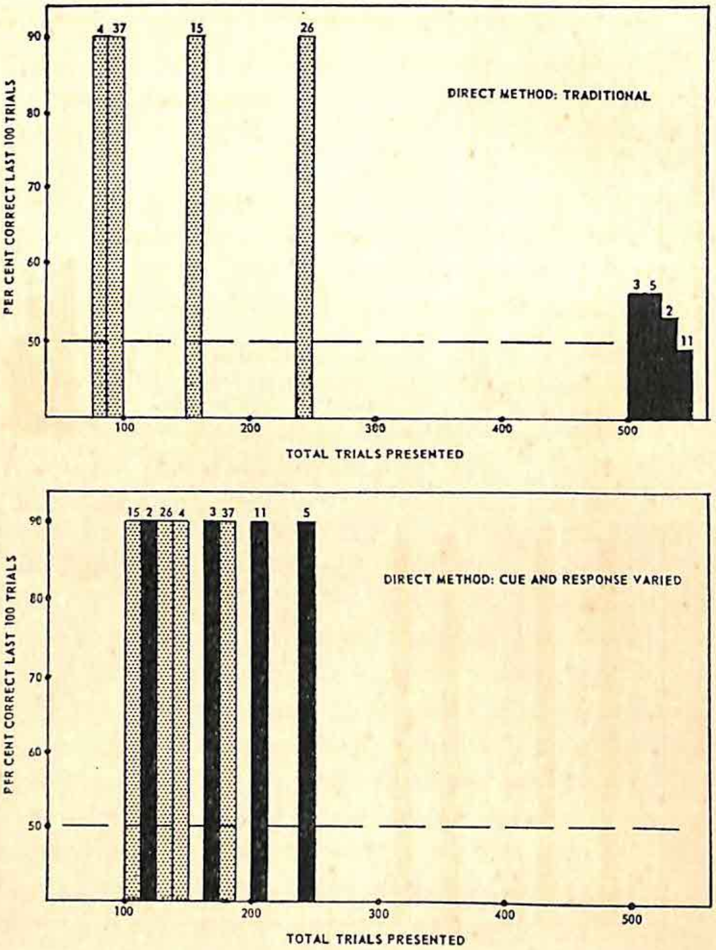


FIGURE 11. Bar graph comparing the performance of anterofrontal and control (inferotemporal) operates on two types of delayed reaction. Each bar represents the performance of one animal (designated by the number above the bar). Note the successful performance of anterofrontal operates (comparable with that of controls) when the method of presentation of predelay cues and opportunity for response are both changed from a simultaneous, right-left situation (upper diagram) to a successive, go—no go situation (lower diagram).[7]

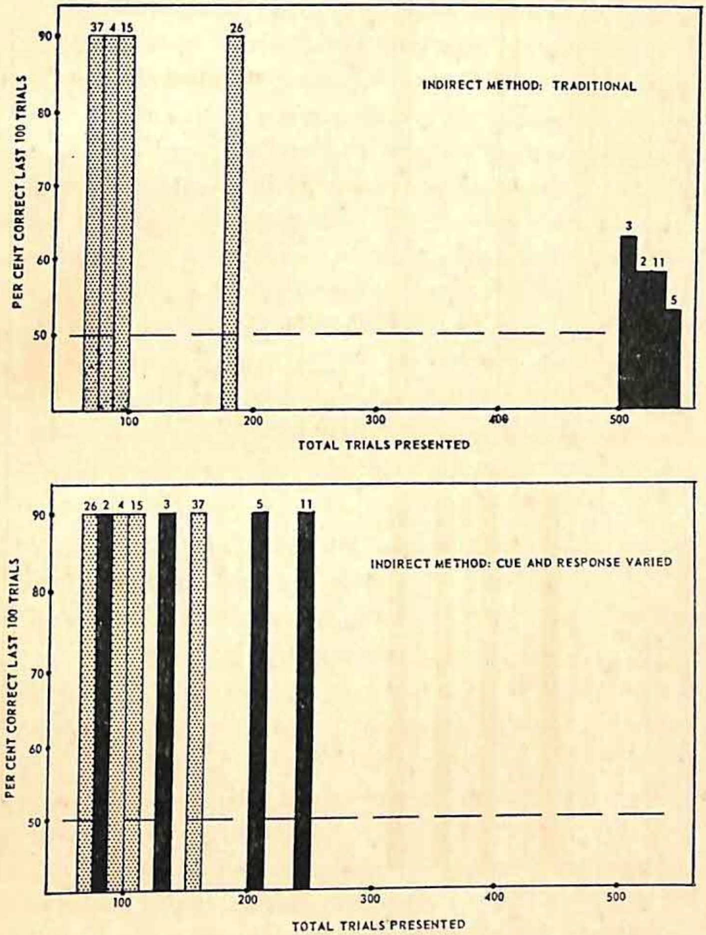


FIGURE 12. Same as Figure 11 except that the indirect method of cueing was used. Results are comparable to those obtained when peanuts are used (direct method).[7]

tunity for opening or not opening a single centered cup, animals with frontal lesions perform almost as well as their controls. When either the predelay cues or response conditions are varied alone, such dramatic improvement of frontal operates' performance does not take place. However, as can be seen from Figure 13, manipulations of the predelay cue are markedly more effective than manipulations of the response conditions. In these experiments when the predelay cue was changed from a spatial to a non-spatial one, frontal operates' performance improved. This might have been the result of changes in the spatial aspect of the discriminans. On the other hand, the relevant change might be the fact that for monkeys the peanuts and objects used as predelay cues had acquired greater "distinctiveness" during prior testing than is possible with a right-left choice. Comparing performance on another task, spatial alternation, which is also consistently failed by anterofrontal operates, with these animals' performance in a non-spatial object alternation, should answer the question of whether spatiality or "acquired distinctiveness" of cues is the relevant variable accounting for the improved performance of the above tasks. Figure 14 compares performance in 1,000 trials of anterofrontal operates and control operates in spatial and object alternation.[12] As can be seen, frontal operates *are* impaired in their performance of both tasks. Thus, spatiality per se cannot be the relevant predelay cue dimension responsible for anterofrontal operates' failure in delayed-response type tasks. Rather, the result of this experiment suggests the hypothesis that the remarkably high level of performance achieved by frontal operates on certain variations of delayed response are due to the "distinctiveness" which the pre-

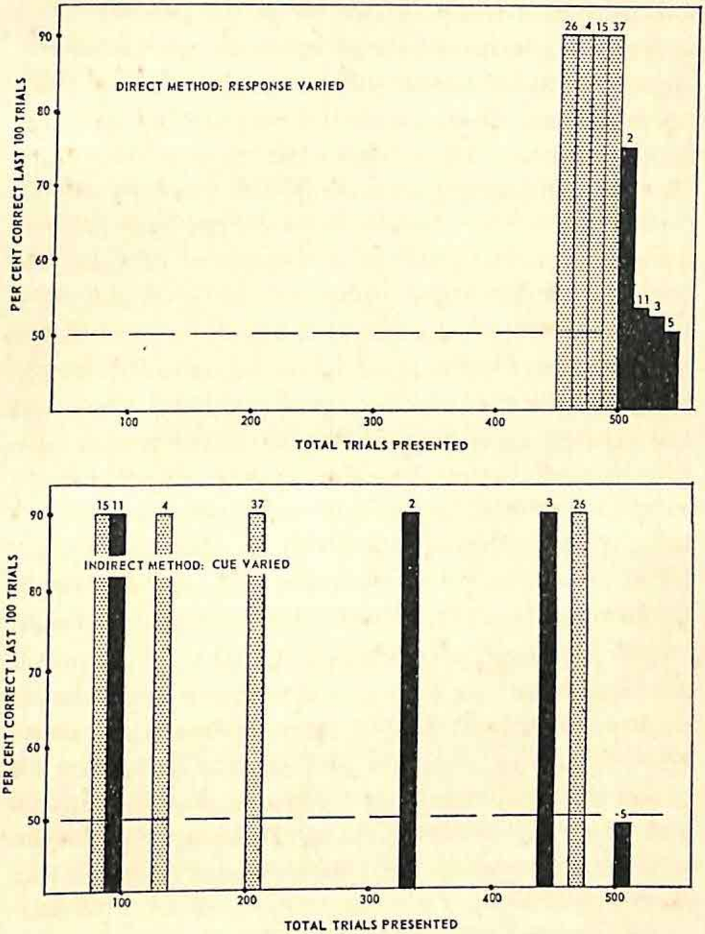


FIGURE 13. Bar graph comparing the performance of anterofrontal and control (inferotemporal) operates on further variations of the delayed reaction task. The upper graph represents performance when cues are presented in right-left positions as in the classical method but opportunity for response is go—no go as in the successive method. The lower diagram represents performance when cues are presented successively but opportunity for response is unchanged from that used in the classical method (go right—go left).[7]

delay cues had acquired during prior training. Conversely, performance decrement, when present in such animals, must be considered a function of the distinctiveness of the predelay cue. Thus, frontal operates' impairment on classical delayed response is a function, not only of the delay, but also of the distinctiveness of the predelay cue.

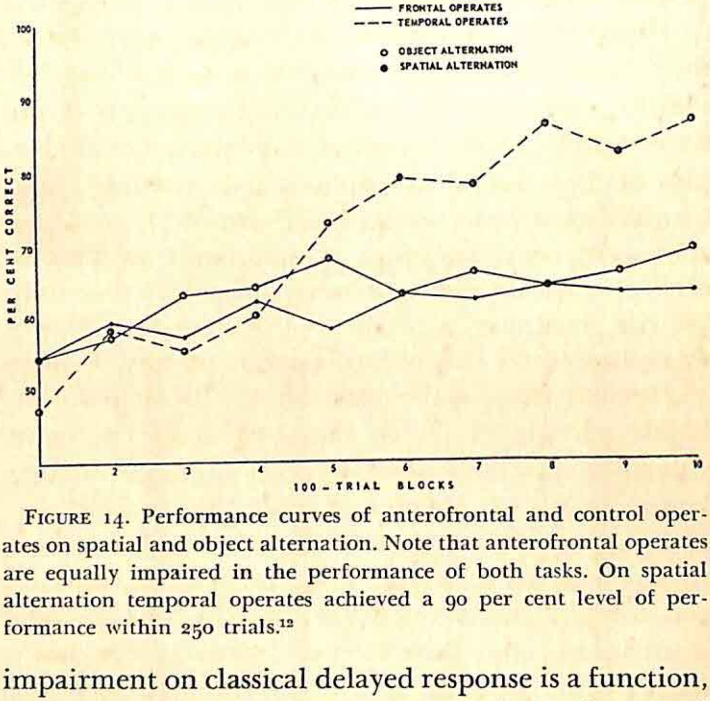


FIGURE 14. Performance curves of anterofrontal and control operates on spatial and object alternation. Note that anterofrontal operates are equally impaired in the performance of both tasks. On spatial alternation temporal operates achieved a 90 per cent level of performance within 250 trials.[12]

The impairment in choice behavior which follows lesions of both intrinsic systems discussed is, therefore, a function of distinctiveness of cues. The effects of resection of the posterior (inferotemporal) system and the anterior (anterofrontal) are *distinguished* in terms of other relevant variables: The posterior system has been related to discrimination behavior which is modality specific; the anterior system to discriminations made in the presence of a temporal gap between cue presentation and response.

At this point I should like to turn from the intrinsic systems. Since one of the functions of this symposium is to discuss the relation of all of the behavioral sciences, the following experiment is apropos. In this instance, the surgical manipulation involved a portion of the second rhinencephalic system,[18] the amygdaloid complex of the cerebral hemisphere. The environmental manipulation concerned a social group of eight preadolescent male macaques. A dominance ranking of each animal with respect to other animals in the group (during feeding) was obtained prior to surgery. Figure 15 demonstrates this preoperative hierarchy. Figures 16, 17, and 18 show the effect on this hierarchy of bilateral amygdalectomy of the three most dominant animals (one animal operated on at a time). Although all lesions are of comparable extent, there are differences among the operates in direction and degree of change in social behavior. Thus, Dave drops from the #1 position to become #8; Zeke, who became the dominant animal after Dave's demise, was also sent downward in the hierarchy by the resection. Riva, Zeke's successor, however, met with no such fate. On the basis of this and subsequent experiments in which relevant variables were manipulated separately, it appears that the amount of aggressive behavior displayed by the #2 animal toward the operate during the immediate postoperative period may be critical in determining the effect of amygdalectomy. Thus, as in the case of the intrinsic systems, complete description of the effects

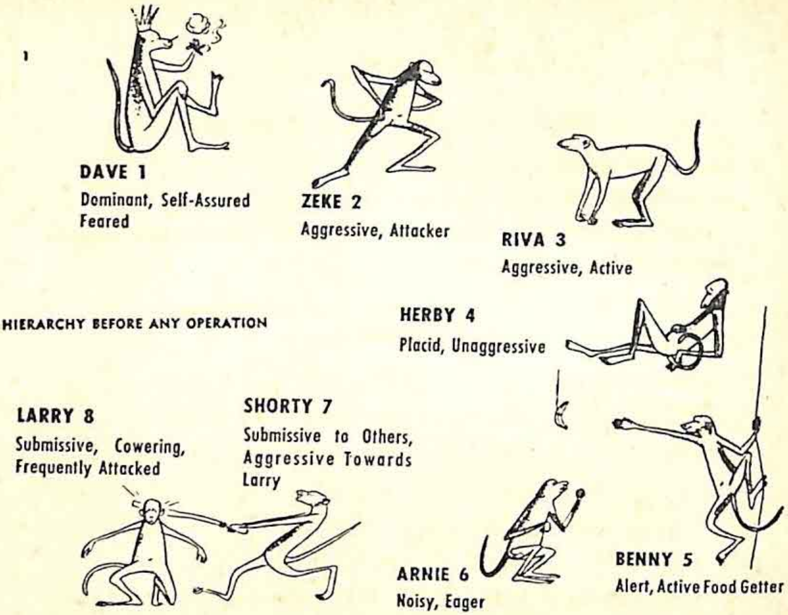


FIGURE 15. Dominance hierarchy of a colony of eight preadolescent male rhesus monkeys before any surgical intervention.[18]

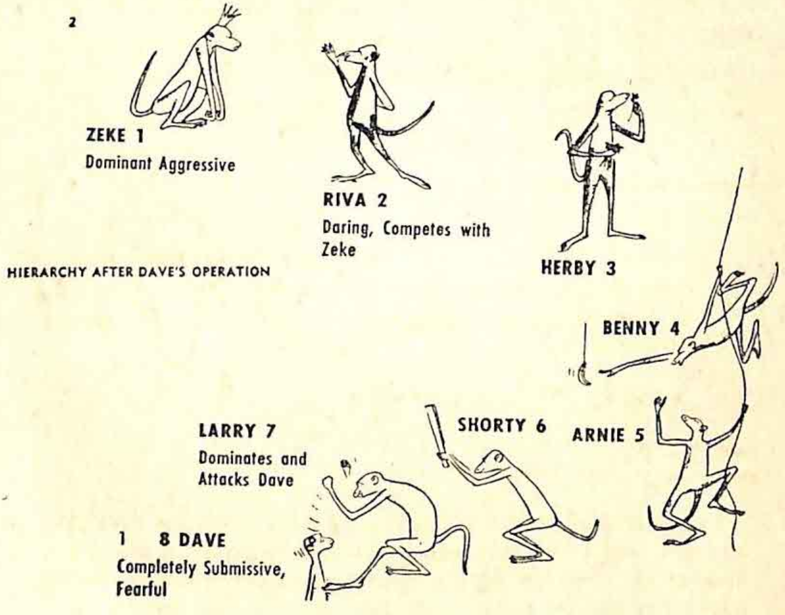


FIGURE 16. Same as Figure 15 after bilateral amygdalectomy had been performed on Dave. Note his drop to the bottom of the hierarchy.

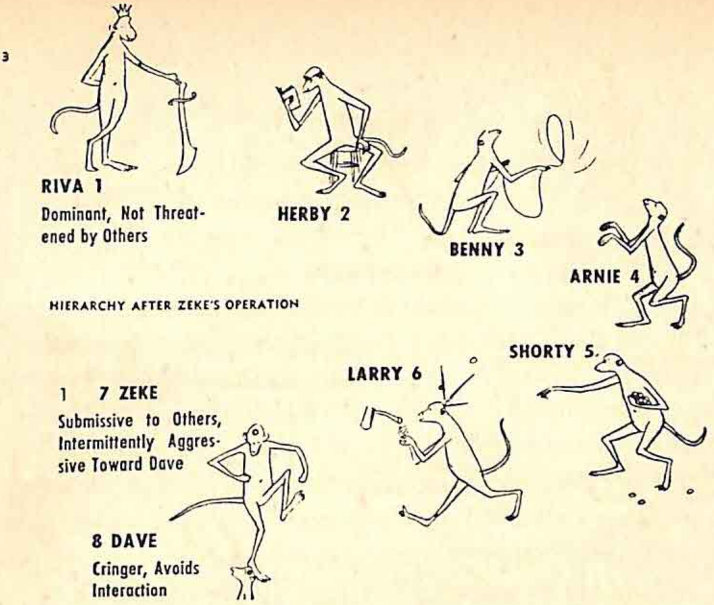

3
RIVA 1
Dominant, Not Threatened by Others
HERBY 2
BENNY 3
ARNIE 4
HIERARCHY AFTER ZEKE'S OPERATION
SHORTY 5.
LARRY 6
1 7 ZEKE
Submissive to Others, Intermittently Aggressive Toward Dave
8 DAVE
Cringer, Avoids Interaction

FIGURE 17. Same as Figure 15 and Figure 16 except that both Dave and Zeke have received bilateral amygdalectomies.

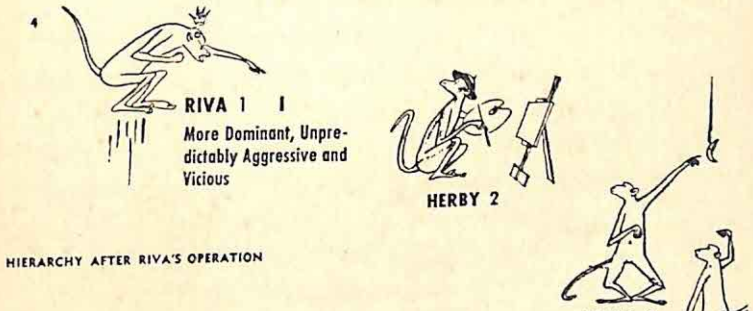

4
RIVA 1 I
More Dominant, Unpredictably Aggressive and Vicious
HERBY 2
HIERARCHY AFTER RIVA'S OPERATION
SHORTY 5
BENNY 3
ZEKE 7
Continues Intermittently Aggressive Toward Dave
LARRY 6
ARNIE 4
DAVE 8
Outcast, Flees From All

FIGURE 18. Final social hierarchy after Dave, Zeke, and Riva have all had bilateral amygdalectomies. Minimal differences in extent and locus of the resections do not correlate with differences in the behavioral results. As noted in the text, Herby's nonaggressive "personality" in the #2 position of the hierarchy seems the most likely explanation of the disparate effects of similar lesions.[18]

of brain lesions *must* include specification of the environmental variables which determine the changes in behavior.

*In Summary:* As indicated in the title of this presentation, I have discussed method and data (relations between dependent and independent variables) which may lead toward a science of neuropsychology. Conspicuously undeveloped are the laws (relationships to a dependent variable of classes of interrelated independent variables) which form the substance of any science. I feel, perhaps erroneously, that there is, as yet, an insufficient scope of data to allow the formulation of general laws. However, some of the terms which must be included in any rigorous formulation are being uncovered.

As an example, some cerebral systems have been surgically manipulated on the basis of neuroanatomical and neurophysiological data and some relationships to behavior have been described. The cortex of these systems has previously been referred to as "associative" on the basis of presumed anatomical connections, physiological "silence," and "clinical" observation. The experiments described offer one method of delineating more precisely the role of these systems in behavior. The inferotemporal sector has been selectively related to performance of visual choice reactions. Resections of this sector result in impairment of visual choice reactions, the impairment being proportional to the distinctiveness of the discrimination as defined by the difficulty of the task for control animals. The dimension of "distinctiveness" is related not only to the physical parameters of the cue, but also to some "noncue" (situational) variables determining the response.

A second example concerned the relationship of the anterofrontal sector to choice behavior dependent on cues not present at the time of response. Experiments have been reviewed which show that this relationship is insufficiently described by the parameter of temporal contiguity between pre- and postdelay contingencies, and that "distinctiveness" of the predelay cue is as important a variable as "time." Thus, the effects of lesions of both intrinsic systems discussed are a function of the distinctiveness of the cues upon which the choice behavior is dependent. The effects of lesions of the posterior and anterior systems may be distinguished, however, by other relevant variables: The posterior lesion has an effect which is modality specific; the anterior lesion is effective only when choice is dependent on cues temporally remote from the response.

The third example concerned one of the rhinencephalic systems and showed that specification and manipulation of environmental variables is as important in understanding the relation between brain and social-emotional behavior as in understanding such a relationship to choice behavior. The example showed that comparable lesions of the amygdaloid complex resulted in diverse effects on the dominance of a #1 animal in a social hierarchy depending on the amount of aggressive interaction with the #2 animal during the immediate postoperative period.

Accumulation of data according to the approach presented here should make possible, in the future, a systematization of relationships between neurological and behavioral data which will constitute a science of neuropsychology. Though the development of this science is dependent on the development of neurology and psychology, the reductive sibling may be expected to add impetus to the growth of its less hybrid sister sciences. At present, data gathering, guided by hypotheses, fills our time and capacity. It is my hope that the results of these endeavors may stimulate others to join in this approach, for, only when data sufficient in range and scope are available, will the formulations which constitute a science be possible. Our particular science, neuropsychology, has a special role to fill at this time: The largest gap in our conceptualizations lies between the behavioral and the physiological sciences —a gap paralleling that which existed between the physiological and physical sciences a century ago. A common framework for the physical and physiological sciences resulted from experiments such as the synthesis of urea—from neuropsychological experiments we may expect the emergence of a common framework relating physiological and behavioral science.

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### *Acknowledgment*

I wish to express my gratitude to my colleagues, Lawrence Weiskrantz, Lawrence Kruger, William Wilson, and Margaret Varley, for their help in generating the ideas set forth here. Stimulating suggestions have also come from conversations with K. S. Lashley, S. S. Stevens, B. F. Skinner, W. A. Rosenblith, H. L. Teuber, and E. I. Burdock. Special thanks are due Mortimer Mishkin whose continuing collaboration throughout the studies quoted made this manuscript possible.

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